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ARTICLES

American Political Science Review Vol. 87, No. 4 December 1993

THE EFFECTIVENESS OF ANTITERRORISM POLICIES: A VECTOR-AUTOREGRESSION-INTERVENTION ANALYSIS

WALTER ENDERS and TODD SANDLER *Iowa State University*

Using quarterly data from 1968 to 1988, we analyze the time series properties of the various attack modes used by transnational terrorists. Combining vector autoregression and intervention analysis, we find strong evidence of both substitutes and complements among the attack modes. We also evaluate the effectiveness of six policies designed to thwart terrorism. The existence of complements and substitutes means that policies designed to reduce one type of attack may affect other attack modes. For example, the installation of metal detectors in airports reduced skyjackings and diplomatic incidents but increased other kinds of hostage attacks (barricade missions, kidnappings) and assassinations. In the long run, embassy fortification decreased barricade missions but increased assassinations. The Reagan "get tough" policy, which resulted in the enactment of two laws in 1984 and a retaliatory raid on Libya in 1986, did not have any noticeable long-term effect on curbing terrorist attacks directed against U.S. interests.

Despite its recent decline, transnational terrorism still poses a real threat to nations worldwide. Transnational terrorism has followed a cyclical pattern; hence, the recent downturn is apt to be followed by an upturn (Im, Cauley, and Sandler 1987). A question of considerable importance concerns what antiterrorism policies have worked best in curbing terrorism. For example, how effective are metal detectors in airports? Do these metal detectors have unintended consequences on other types of terrorist attacks? Are U.S. diplomats safer since their embassies have been made more secure? Are U.S. antiterrorism laws effective? Did the U.S. raid on Libya in April 1986 reduce attacks against U.S. interests? We shall attempt to provide answers to these questions.

Terrorism is the premeditated use—or threatened use—of extranormal violence or force to gain a political objective through intimidation or fear. Terrorists often direct their violence and threats at a large target group, not immediately involved in the political decision-making process that they intend to influence. The larger the target group, the more difficult it is for authorities to anticipate the next attack. To bring pressures on decision makers to acquiesce to their demands, terrorists massacre innocent tourists in foreign airports, take other nations' citizens hostage, or bomb military and civilian targets. Terrorists wage their campaigns to achieve diverse political goals that may be founded on nationalistic, ideological, issue-specific, separatist, revolutionary, or nihilistic grounds. Whatever its motivation, terrorism imposes costs on governments. Terrorist groups may act independently, in a coalition with other groups (e.g., the American Battalion in South America), or as a state surrogate.[1] Targeted governments must decide how best to allocate resources to thwart terrorism, while the terrorists and their sponsors must determine what modes of attack to include in their campaigns in light of government actions. On both sides, economic decisions are required.

Both passive and active responses have been proposed by terrorist experts to thwart terrorism (see, e.g., Tovar 1986; Wilkinson 1986). Passive or defensive responses include erecting technology-based barriers (e.g., bomb-sniffing devices, metal detectors), hardening targets, instituting stricter laws and penalties, increasing resource commitments, and enacting international agreements. Active responses call for retaliatory raids (especially against state sponsors), preemptive strikes, group infiltration, and covert actions. In terms of U.S. antiterrorism policies, three passive and one active policy have been used since the early 1970s. Passive policies have included the installation of metal detectors in airports in January 1973 (a policy copied worldwide shortly after the U.S. lead), the fortification of U.S. embassies and military installations starting in 1976, and the passing of antiterrorism laws in 1984. The most noteworthy active response was the April 1986 U.S. retaliatory raid on Libya.

In recent years, economists have begun to assess the effectiveness of these and other policies aimed at curbing terrorism. For example, Landes (1978) used ordinary least squares regression techniques to assess the effectiveness of metal detectors, sky marshals, stiffer penalties, and intelligence profiles on skyjackings originating in the United States in the late 1960s and early 1970s. Cauley and Im (1988) applied intervention or interrupted time series analysis (see McCleary and Hay 1980) to determine the impact of metal detectors, increased expenditures to harden U.S. embassies in 1976, and the UN convention on preventing attacks against protected persons. Enders, Sandler, and Cauley (1990) refined the application of intervention analysis and focused their policy evaluation on a host of UN conventions and international responses to hijackings. Brophy-Baermann and Conybeare (n.d.) employed intervention analysis to examine the short-run and long-run effects of Israeli retaliations on the rate of terrorist attacks. Each of the previous intervention studies analyzed the effects of one or two policies on the time series of a *single mode of attack; no interactions among series were allowed.*[2]

Since terrorists must weigh the relative costs and benefits of various modes of attack when designing their campaigns, we should expect that the time series for one mode (or target) of attack may depend not only on the time series for that mode but also on those of other related modes. A primary purpose of this article is to employ vector autoregression (VAR) techniques to identify the interrelationships among the time series for different kinds of terrorist events. These interrelationships should assist us to identify modes that behave as *substitutes* or *complements* for one another. Two modes of attack are substitutes if they fulfill similar purposes (e.g., all types of hostage missions raise extortion). Modes are complements if they enhance one another's effectiveness (e.g., threats and actual events promote an atmosphere of fear; high-profile skyjackings and diplomatic attacks reinforce the public's perception of a government's vulnerabilities). A second purpose is to investigate the short-run and long-run impact of alternative policies on terrorist tactics and activity levels when the interrelationships between various modes are explicitly taken into account. To accomplish this second purpose, we engineer a procedure for combining VAR and intervention analysis. By indicating the impact of a policy on a host of terrorist attack modes, this procedure better enables us to evaluate the costs and benefits associated with a policy. If, for example, the installation of metal detectors permanently reduces skyjackings but encourages other kinds of hostage-taking missions, then the true net benefits of metal detectors must account for both effects and any other indirect impacts. The ability to identify interrelated time series would also assist in forecasting future trends in terrorism, especially if a contemplated intervention is predicted to affect one terrorist tactic or mode of attack directly.

Using quarterly data on transnational terrorist events from 1968 to 1988, we evaluate the impact of recent U.S. interventions. The most effective policies were the installation of metal detectors in January 1973 and the fortification of U.S. embassies in 1976 and thereafter. Although metal detectors decreased skyjackings, they had the unintended effect of increasing other types of hostage missions and assassinations. Significant interrelationships among series are uncovered that influence the impact of antiterrorism policies. For example, metal detectors appeared to decrease threats and hoaxes, perhaps owing to their reduced credibility. The Libyan raid led to a short-term increase in U.S. and British attacks that spilled over to other countries' interests. No long-run decrease in terrorism could be attributed to the raid. Finally, the Reagan antiterrorism laws did not inhibit terrorism directed against U.S. interests.

CHOICE-THEORETICAL CONSIDERATIONS

Terrorists are viewed here as rational actors who attempt to maximize a *shared goal,* subject to a resource constraint. This shared goal may be denoted as utility or expected utility derived from the consumption of *basic commodities,*[3] produced from terrorist and nonterrorist activities. For example, a nihilistic group may gain utility from political instability, which is a basic commodity derived from various terrorist tactics or modes of attack (e.g., armed attack, bombings, hostage taking, threats). If the government appears ineffective in curbing these acts or if the government overreacts and appears repressive, then political instability results. Substitution possibilities among tactics arise, inasmuch as some terrorist tactic may produce the same basic commodities (e.g., extortion, media attention) but in varying amounts. Substitution is enhanced when attack modes with closely related outcomes are logistically similar. Hostage-taking events, such as skyjackings, kidnappings, and barricade missions, are logistically complex, and this augments substitution possibilities. Assassinations also require a good deal of planning, but less than hostage missions. In contrast, bombings and non-resource-using events (e.g., threats and hoaxes) are logistically simple. Complementarity results when combinations of attack modes are needed to produce one or more basic commodities. That is, combining attack modes may have greater marginal productivity in producing basic commodities than either mode individually. For example, threats when joined with actual attacks may create a greater atmosphere of fear than either alone. Skyjackings when combined with attacks against protected persons (e.g., diplomats and military officials) may make a government appear weaker than reliance on a single mode of attack. To produce these basic commodities, a terrorist group has at least three allocative decisions to make:[4] (1) a choice between terrorist and nonterrorist activities, (2) a choice between different terrorist tactics, and (3) a choice between different nonterrorist activities. The second choice involves a decision among skyjackings, bombings, and other attack modes. It also requires a choice of whom to attack—U.S. interests or those of another nation.

A resource constraint limits the terrorist group's expenditures on activities not to exceed its income or resources. The expenditures on any activity consist of the product of the activity's per-unit price and the level of the activity. Each terrorist tactic has a per-unit price that includes the value of time and resources needed to accomplish the act. Given its logistical simplicity, a bombing may well involve a lower price than the kidnapping and maintenance of a hostage. The prices faced by the terrorists for each tactic are determined, in large part, by the government's allocation of resources to thwarting various acts of terrorism. If, for example, the government were to fortify its embassies, then attacks against embassies and diplomats within the missions' grounds would become more difficult. This, in turn, means that the per-unit price of such activities has risen. Similarly, the installation of metal detectors increases the per-unit and relative price of skyjackings when compared with assassinations or other types of hostage incidents. Any government policy directed at a single type of terrorist event would change relative prices and should result in substitution between modes. If, however, the government wants to induce the terrorists to rely more on nonterrorist tactics, then the government must intervene with policies that either raise the price of all types of terrorist modes or lower the price of nonterrorist activities (e.g., easier access to elections).

By casting the terrorist group's allocation problem in a household production framework, we can state some easily derivable propositions:

PROPOSITION 1. *An increase in the relative price of one type of terrorist tactic would cause a terrorist group to substitute out of the now-more-costly mode into those terrorist and nonterrorist activities whose prices are now relatively* less *costly.*

PROPOSITION 2. *Terrorist events yielding similar basic commodities should display the greatest substitution possibilities. For example, more substitution is predicted between hostage-taking missions than with other types of events when one kind of hostage-taking mission's relative price has increased. If income effects are not too strong, then complementary events would respond in a similar fashion to relative price changes; that is, the increase (decrease) in the price of one complementary event causes that event and all complements to fall (rise) in number.*

PROPOSITION 3. *A decrease in all terrorist activities would result from government interventions that raised the price of all terrorist tactics relative to nonterrorist activities.*

PROPOSITION 4. *For normal goods, an increase (decrease) in its resource endowment would cause the terrorist group to increase (decrease) the level of terrorist and nonterrorist activities.*

Propositions 1–3 indicate the dilemma confronting governments. If the authorities concentrate efforts on thwarting a single terrorist tactic through, say, increased security, the terrorist will substitute into other tactics. Proposition 2 indicates the kinds of substitutions expected. Propositions 3 and 4 indicate that government policies must either raise costs to terrorist tactics across the board or limit terrorist resources if overall terrorism can be anticipated to fall. We provide evidence of substitution-and-complementarity phenomenon in our empirical results section.

STATISTICAL METHODS AND DATA

Our basic methodology combines intervention analysis in a VAR framework.[5] The data were taken from two primary sources containing chronologies of transnational terrorist incidents (Mickolus 1980; Mickolus, Sandler, and Murdock 1989).

Methodology

In order to explain the analytical procedure, we first consider a streamlined model in which there are only two attack modes and two policy instruments. Consider the following VAR model using only one lagged value of each attack mode:

$$y_1(t) = a_{10} + a_{11}p_1(t) + a_{12}p_2(t) + b_{11}y_1(t-1) + b_{12}y_2(t-1) + \epsilon_1(t) \quad \textbf{(1)}$$

$$y_2(t) = a_{20} + a_{21}p_1(t) + a_{22}p_2(t) + b_{21}y_1(t-1) + b_{22}y_2(t-1) + \epsilon_2(t) \quad \textbf{(2)}$$

where $y_i(t)$ is the number of type i terrorist incidents occurring during time period t, $p_i(t)$ is an indicator of whether policy i is in effect at time t, $\epsilon_i(t)$ is an identically and independently distributed random variable with mean equal to zero, and the a_{ij}s and b_{ij}s are parameters. Note that $\epsilon_1(t)$ and $\epsilon_2(t)$ may be correlated.

To illustrate the meaning of equations 1 and 2, recall that metal detectors were installed in U.S. airports on 5 January 1973. If policy intervention 1 represents metal detectors, the value of $p_1(t)$ can be set equal to zero for all $t <$ January 1973 and equal to unity for all $t \geq$ January 1973. Hence, the coefficients a_{11} and a_{21} measure the direct-impact effect of metal detectors on attack modes 1 and 2, respectively. It is important to note that even if a_{i1} is found to equal zero, it is *not* possible to conclude that incident type i was unaffected by policy intervention 1. As long as b_{12} and/or b_{21} are not zero, there are interactions among the two incident types. For example, even if $a_{11} = 0$, policy intervention 1 can have an indirect effect on incident type 1 through its effects on incident type 2. The point is that the direct effect of an intervention on a particular mode of attack may be zero even though the indirect effects may be nonzero. This observation motivates the need to study terrorist-thwarting interventions in a VAR framework as opposed to the standard univariate intervention model.

To generalize the discussion to the case of additional attack modes, policy interventions and lags, we consider:

$$y(t) = \alpha + AP(t) + \sum_{\ell=1}^{L} B_\ell y(t-\ell) + \epsilon(t), \quad \textbf{(3)}$$

where $y(t)$ is the $n \times 1$ vector $[y_1(t), y_2(t), \ldots, y_n(t)]'$; $y_i(t)$ is the number of type i terrorist incidents occurring during time period t; α is a 1×4 matrix consisting of a constant and three seasonal dummy variables; the B_ℓ's are $n \times n$ coefficient matrices; and $\epsilon(t)$ is the $n \times 1$ vector $[\epsilon_1(t), \epsilon_2(t), \ldots, \epsilon_n(t)]'$, such that $\epsilon_i(t)$ is *identically and independently distributed* (i.i.d.). Note that the correlation matrix of the shocks is not necessarily diagonal. In equation 3, matrices A and $P(t)$ are of particular importance for assessing the success of the alternative interventions. $P(t)$ is the $k \times 1$ vector $[p_1(t), \ldots, p_k(t)]'$, where $p_k(t)$ is an indicator of whether policy k is in effect at time t. Each element a_{ik} in the $n \times k$ coefficient matrix A measures the direct-impact effect of policy intervention p_k on the time series of incident type y_i.

As in the simple 2×2 model, even if a_{ik} is equal to zero, it is *not* possible to conclude that incident type i was unaffected by policy intervention p_k. When the off-diagonal elements of the B_ℓ matrices are not zero, there are interactions among the various incident types. Thus, policy intervention p_k may have an indirect effect on incident type i through its effects on a related incident type.

An appropriate strategy to estimate the matrices in equation system 3 is straightforward. An identical procedure is applied in each of our estimated models.

Step 1. Equation system 3 was estimated for lag lengths of eight quarters, four quarters, and two quarters (i.e., $L = 8, 4, 2$). Since each of the equations in equation system 3 has identical right-hand-side variables, ordinary least squares is an efficient estimation technique. Lag lengths are selected using the likelihood ratio test for the restriction that all coefficients in B_ℓ for $\ell > m$ (where $m = 4$ or 2) are equal to zero. The test statistic has a chi-squared distribution with degrees of freedom equal to the number of excluded parameters (i.e., $df = n(8 - m)$).

Step 2. As a preliminary step for ascertaining the importance of the interrelationships among the various attack modes, we obtained the variance decompositions from the moving average representation of the model estimated in step 1. The moving average representation of $y_i(t)$ expresses the number incidents as dependent on the current and past values of all the error terms and interventions.[6] Appropriately shocking the $\epsilon_1(t)$-through-$\epsilon_n(t)$ series, we can obtain the percentage of the variation attributable to each type of attack mode innovation. If two series are highly interrelated, one (or both) will explain a reasonable proportion of the forecast error variance of the other. To account for the possibility of short-term-versus-long-term interactions, we consider 4-step-, 8-step-, and 24-step-ahead forecasting horizons. When the error terms of the equations in equation system 3 are correlated with each other, there is no simple way to obtain a pure shock to any one of the series. In those instances where the estimated errors of any two series are correlated, we follow the usual practice and perform a second variance decomposition reversing the ordering of the variables involved.[7]

Step 3. The forecasts from an unrestricted VAR are known to suffer from "overparameterization." In order to obtain a more parsimonious model, we used the results of the variance decompositions. Many of the series explained the preponderance of their own forecast error variance; for such variables, we used a simple two-step procedure to pare down the number of estimated coefficients. If incident type y_j did not explain at least 10% of the forecast error variance of incident type y_i at any of the three forecast horizons, we then considered the results of Granger-causality tests. Formally, the restriction that incident type y_j does not Granger-cause incident type y_i can be written as

$$b_{1ij} = b_{2ij} = \ldots = b_{Lij} = 0 \ (i \neq j), \tag{4}$$

where $b_{\ell ij}$ is element ij of matrix B_ℓ. If we could not reject the null hypothesis that incident type y_j does not Granger-cause incident y_i at the .10 level of significance, we imposed the restriction given in equation 4. Thus, the restriction was imposed only in those instances in which y_j explains less than 10% of the forecast error variance of y_i and y_j does not Granger-cause y_i at the .10 level of significance.

Step 4. We next reestimated equation system 3, imposing the zero-restrictions implied by the causality tests. Since the right-hand-side variables were no longer identical in each regression, each was estimated using *seemingly unrelated regressions* (SUR).

Step 5. The SUR estimates of the A and B_ℓ matrices can be used to show the effectiveness (or lack thereof) of the various interventions. Each element in A shows only the impact effect of the intervention. The lagged impact and indirect effects are captured by the elements in the B_ℓ matrices. Because of the complexity of the interactions, the simplest way to demonstrate the short- and long-run policy effects is through the moving average representation.[8]

Step 6. Steps 1–5 were conducted for all three of our models. Our diagnostic checking included using the Ljung–Box Q-statistic to check for serial correlation in the residuals. We were also careful to look for signs of nonstationarity. In model 3, estimated coefficients and diagnostic checks led us to question the assumption that all of the series were stationary. The nonstationarity issue is difficult to address in a system of equations with multiple interventions. A variable may appear to be nonstationary owing to the effects of successive interventions. We visually inspected the time paths of each type of terrorist incident, being careful to note any evidence of nonstationary behavior. In addition, the estimated coefficients from the VAR and the forecast values from the time paths from step 5 were investigated. For reasons that we associate with the introduction of metal detectors in airports, most of the series exhibit a break immediately following the first quarter of 1973. For the post-1973 period, we formally tested each suspected series for the possibility of a unit root using the augmented Dickey–Fuller tests. If a series contains a unit root, we use its first-difference in the analysis.[9]

Data

The chronological data include the agents, victims, territories or institutions of two or more nations (Mickolus 1980; Mickolus, Sandler, and Murdock 1989). These chronologies are available as coded data sets entitled International Terrorism: Attributes of Terrorist Events (ITERATE). ITERATE 2 contains the attributes of international terrorist events from 1968 to 1977 (Mickolus 1982). ITERATE 3 contains these attributes from 1978 to 1987 (Mickolus, Sandler, Murdock, and Fleming 1989).[10] The data for 1988 were drawn from a chronology update written by Edward Mickolus and made available to us. These chronologies represent the most comprehensive data set publicly available. ITERATE draws its data from publicly available materials that have appeared in the world press. Key sources include the Associated Press, United Press International, Reuter tickers, the *Washington Post,* the *New York Times,* the *Washington Times,* and the Foreign Broadcast Information Service (FBIS). The most valuable source for ITERATE is the regional FBIS *Daily Reports,* which draws from hundreds of world print and electronic media resources and is the best single source of material on foreign coverage of terrorist incidents. The FBIS reports include important print and media sources that appear in English, Arabic, and other languages.

Germane to this study, the chronologies record the type of event, date, location, and the victims' or targets' characteristics (e.g., what nationality and whether a diplomat or not). Consistency is maintained since identical criteria for defining acts of transnational terrorism and classifying types of incidents are applied to the entire data set, which includes 84 quarters of observations from 1 January 1968 to 31 December 1988.

In total, 15 primary time series were extracted from the data. These time series consisted of the quarterly totals, or count, of events of a given type. The following basic series were constructed: (1) assassinations, (2) barricade-and-hostage-taking events, (3) kidnappings, (4) transnational skyjackings, (5) U.S. domestic skyjackings, (6) other skyjackings, (7) total terrorist incidents, (8) nonhostage crimes against diplomats and protected persons ("protected-persons events"), (9) crimes against U.S. protected persons, (10) crimes against British protected persons, (11) attacks against U.S. persons and property, (12) attacks against British persons and property, (13) all crimes against protected persons, (14) threats, and (15) hoaxes. Other time series were manufactured from these basic series. United States domestic skyjackings involved hijackings within the United States and hijackings originating in the United States in which the hijacker demanded to be flown to Cuba. Most of these hijackings were not politically based but were included to test for the effectiveness of metal detectors and screening devices. Other skyjackings involved domestic skyjackings *outside* the United States and skyjackings for political asylum and nonpolitical motives. Again, these were included to determine metal detectors' effectiveness. Transnational skyjackings fit ITERATE's definition of terrorism.[11] The total terrorism series included domestic and other skyjackings along with all legitimate terrorist events. The crimes-against-protected-persons series followed the United Nations' definition for protected persons (1978, 76–77). Attacks were counted if they occurred in a country signing the UN convention on protected persons. Moreover, the attack had to involve victims from a signatory country. Threats concerned claims of future acts with no subsequent followup, while hoaxes referred to an alleged past act (e.g., a bomb planted on a plane) that never took place.

The VAR technique requires that there be no overlap among series. Hence, for the statistical models, we had to purge the overlap. In model 1, for example, the crimes-against-protected-persons series did not include any such events as occurred in the barricade, kidnapping, or skyjacking modes. This, then, permitted us to run skyjackings and other kinds of hostage events as separate series to examine interrelationships among time series. Each of the three statistical models presented have an "other events" series. In models 1 and 2, "other events" primarily included bombings and armed attacks. In model 1, "other events" excluded all hostage and protected-persons incidents. In model 2, "other events" excluded hostage events, assassinations, and threats. Finally, "other events" for model 3 concerned incidents not directed at the United States, the United Kingdom, or protected persons. For some models, we combined two or more time series of a similar nature to avoid too many zero observations and to ensure that the values of the observations were sufficiently large to justify the assumption of normally distributed disturbances (Harvey and Fernandes 1989).

THE INTERVENTIONS

Five passive interventions and one active intervention are investigated with the VAR models. If a policy occurred during the middle of a month (e.g., the Libya retaliatory strike on 15 April 1986), the intervention date corresponds to the start of the nearest quarter (i.e., the second quarter). The first intervention concerns the installation of metal detectors in U.S. airports on 5 January 1973 and is denoted by METAL. Shortly thereafter, these devices were placed in airports worldwide to screen domestic and international flights. Screening devices represented a permanent intervention with continual updates thereafter. The next three interventions involve resource allocations to fortify and secure U.S. embassies and missions in October 1976, 1985, and 1986 and are indicated by EMBASSY76, EMBASSY85, and EMBASSY86, respectively. In 1976, spending on embassy security more than doubled. Significant additional resources were allocated to security in the 1980s as a result of the takeover of the U.S. embassy in Tehran on 4 November 1979. Security measures included the installation of metal detectors to screen visitors to embassies. Large increases in security were authorized to start on October 1985 by Public Law 98-533. On 12 August 1986, the U.S. Congress passed legislation authorizing an additional $2.4 billion over five years to rebuild and fortify U.S. embassies and missions. Embassy fortification was also a permanent intervention.

The fifth intervention concerns two U.S. laws enacted in October 1984. Public Law 98-473, signed by President Reagan on 12 October 1984, required up to life imprisonment for individuals taking U.S. hostages *either within or outside* the United States. Stiffer penalities were included for destroying aircraft or airport facilities within the United States. In addition, penalties were raised for acts committed with a bomb or other weapon on a U.S. aircraft. Public Law 98-533, signed on 19 October 1984, authorized the U.S. attorney general to pay rewards for information leading to the capture or conviction, inside or outside the United States, of terrorists who targetted U.S. interests (Celmer 1987; Pearl 1987, 141). We refer to these laws as part of the Reagan "get-tough" policy and denote them as REAGAN LAWS.

The sixth intervention is the U.S. retaliatory bombing of Libya on the morning of 15 April 1986 for its alleged involvement in the terrorist bombing of the LaBelle Discothèque in West Berlin on 5 April 1986. Since 18 of the F-111 fighter–bombers used in the raid were deployed from British bases at Lakenheath and Upper Heyford, England, the United Kingdom implicitly assisted in the raid. The grounds for retaliatory strikes were laid by National Security Decision Directive 138, signed by President Reagan on 3 April 1984 (Celmer 1987, 3). This intervention is referred to as LIBYA. By including REAGAN LAWS and LIBYA, we can distinguish the relative effectiveness, if any, of words and deeds.

EMPIRICAL RESULTS

In constructing the three statistical models, we emphasized three criteria: (1) the included time series must contain sufficient observations in each quarter to permit the use of count data; (2) all overlap among series within the same model must be purged; and (3) the reported models must provide information not contained in the other models. To accomplish the first criterion, we combined time series; thus, hostage events *not occurring* as skyjackings were added together. Kidnappings and barricade events were consequently summed in a series denoted by *hostage.* The three types of skyjackings were combined each quarter into a series named *skyjackings.* In addition, all non-resource-using terrorist acts (i.e., threats and hoaxes) were summed into a time series called *threats.* Attacks against U.S. and British interests, not involving protected persons, were denoted as *US, UK attacks.* Finally, each model contained a time series for other attacks that did not occur in one of the highlighted modes. The second criterion was satisfied by systematically assigning any overlap to a single series.

Model 1

Model 1 was designed to focus on hostage-taking events, crimes against protected individuals, and other types of events. Any hostage incident directed against protected persons was included in the hostage series but purged from the protected-persons series to avoid double counting. Since both skyjackings and hostage events were included in model 1, we tested the impact of the following interventions: METAL, EMBASSY76, EMBASSY85, EMBASSY86, and LIBYA.

A lag length of two quarters best characterized the four series. The chi-squared statistic for the restriction that all coefficients on lags greater than four (greater than two) were equal to zero had a significance level of .675 (.667).

Table 1 reports the variance decompositions for model 1 for three different forecasting horizons (4, 8, and 24 quarters). The left-hand side of the table uses the ordering consisting of skyjackings → hostage → protected persons → other, and the right-hand, the reverse ordering. Although each series explains the preponderance of its own forecast error variance for either ordering, there are important linkages between the skyjackings and protected-persons series. With the ordering skyjackings-prior-to-protected-persons, innovations in skyjackings explain about 17% of the forecast error variance in the protected-persons series, while innovations in the latter explain about 6% of the forecast error variance in skyjackings. With the reverse ordering, innovations in skyjackings explain about 4% of the forecast error variance of protected persons, while innovations in the latter explain about 15% of the forecast error variance in skyjackings. The ordering is important, since the correlation coefficient between the errors is .314; clearly, the two series exhibit important comovements.

Although F-tests do not imply Granger-causality between skyjackings and protected-persons series, the variance decompositions imply that the two series do tend to move together. The straightforward implication is that policy interventions affecting one of the series have had effects on the other. Besides these two series, we did not find any strong interrelationships among series in model 1. Imposing the zero restriction of the causality tests, we reestimated the equations using SUR. Specifically, we allowed each series to depend on a constant, three seasonal dummy variables, its own lagged values, and the four interventions. In addition, the protected-persons series was allowed to depend on lagged values of skyjackings, and the skyjackings series was allowed to depend on lagged values of the protected-persons series.

In Table 2, we list the coefficients of the four interventions resulting from the SUR model. On impact, metal detectors decreased skyjackings by 12.2 events per quarter, a result that agrees with other studies (e.g., Cauley and Im 1988). As predicted from a choice-theoretic framework, raising the costs of a skyjacking by introducing metal detectors reduced skyjackings but increased other types of hostage

TABLE 1

Variance Decomposition for Model 1: Percentage of Forecast Error Variance at 4-, 8-, and 24-Quarter Forecast Horizons

ATTACK MODES AND FORECAST HORIZONS	ORDERING FOR TIME SERIES				REVERSE ORDERING FOR TIME SERIES			
	SKY-JACKINGS	HOSTAGE	PROTECTED PERSONS	OTHER	OTHER	PROTECTED PERSONS	HOSTAGE	SKY-JACKINGS
Skyjackings								
4-qu	93.3	1.47	4.87	.332	1.10	14.8	1.93	82.2
8-qu	92.3	1.51	5.67	.478	1.32	15.5	1.97	81.2
24-qu	92.3	1.51	5.67	.479	1.32	15.5	1.97	81.2
	(.156)	(.734)	(.132)	(.911)	(.911)	(.132)	(.734)	(.156)
Hostage								
4-qu	6.13	92.2	.315	1.31	3.71	.460	91.2	4.64
8-qu	6.40	91.1	.879	1.60	4.12	.695	90.1	5.06
24-qu	6.42	91.1	.897	1.61	4.13	.721	90.1	5.07
	(.123)	(.028)	(.948)	(.495)	(.495)	(.948)	(.028)	(.123)
Protected Persons								
4-qu	16.4	1.89	78.1	3.53	6.11	88.9	1.40	3.59
8-qu	17.1	1.88	77.5	3.61	6.14	88.3	1.39	4.21
24-qu	17.1	1.88	77.5	3.61	6.15	88.3	1.39	4.21
	(.191)	(.439)	(.098)	(.436)	(.436)	(.098)	(.439)	(.191)
Other								
4-qu	2.32	7.81	3.56	86.3	91.2	3.68	4.03	1.12
8-qu	2.90	7.84	3.57	85.7	90.5	3.74	4.13	1.60
24-qu	2.91	7.83	3.58	85.7	90.5	3.75	4.13	1.60
	(.677)	(.307)	(.173)	(.005)	(.005)	(.173)	(.307)	(.677)

Note: The numbers in parentheses are the significance levels for the joint hypothesis that all lagged coefficients of the variable in question can be set equal to zero. Quarters are denoted "qu." The underlying data are from ITERATE 2 and 3, and are quarterly observations from 1968:1 to 1988:4.

events. On impact, we estimated that installation of metal detectors led to a significant increase in other kinds of hostage incidents by 3.68 incidents per quarter.

A salient feature of our VAR approach is the finding that metal detectors affected protected persons even though the immediate impact effect of metal detectors on protected persons has a t-statistic of .015. Since the installation of metal detectors reduced skyjackings and since skyjackings affected protected persons, metal detectors indirectly affected the latter. The direct-impact effects alone do not suffice to indicate the effectiveness of the interventions; in a dynamic system, the immediate-impact and long-run effects of the policy actions can be quite different. The lower portion of Table 2 presents the long-run effects of the interventions. An interesting result is that the long-run effect of metal detectors is estimated to reduce protected persons attacks by 4.53 incidents per quarter, even though the immediate impact is .032 incidents. This enhanced long-run effect derives from the interrelationship among the series and from the eventual application of metal detectors to secure protected persons at embassies and military bases.

Three of the four series and the effects from the interventions for model 1 are displayed in Figures 1–3, with incidents per quarter on the vertical axis. The time paths labeled "predicted" are historical effects of the interventions, obtained from the moving average representation of the model. The predicted series are *not* one-step-ahead forecasts. Rather, they are the projections of the $y_i(t)$ series conditioned on the interventions alone; the one-step-ahead forecasts track the actual values quite closely. In Figure 1, the skyjacking series displays a sharp drop with the introduction of metal detectors in 1973; the drop is immediate in that the immediate-impact and long-run effects are quite close (−12.2 and −13.7 incidents per quarter, respectively). The indirect effects of metal detectors on the protected persons series can be seen in Figure 2 where it appears that the effects of metal detectors on protected persons converge to its final level in approximately four quarters. At the same time, other kinds of hostage events steadily increased until 1974 and then remained at this elevated level, as shown in Figure 3. These time paths are consistent with the hypothesis that metal detectors made it more costly for terrorist groups to execute a skyjacking successfully. In consequence, terrorist groups substituted away from skyjackings and complementary events involving protected persons and into other kinds of hostage incidents.

The other major finding in Table 2 is that the Libyan retaliatory bombing led to *increased* bombings and related incidents. Unlike the other interventions, the Libyan raid was modeled as a temporary impact. This raid significantly increased other incidents by

TABLE 2

Coefficients and Significance Levels of the Impacts Effects of Model 1

ANTITERRORISM POLICIES	SKYJACKINGS	HOSTAGE	PROTECTED PERSONS	OTHER
Temporary Effects				
Metal	−12.2**	3.68*	.032	12.2
	(2.06)	(1.62)	(2.08)	(9.91)
Embassy76	2.05	−.441	−1.53	1.73
	(1.25)	(1.24)	(1.26)	(7.12)
Embassy85	−1.72	1.39	.935	−1.48
	(1.41)	(1.48)	(1.42)	(8.13)
Libya	−3.81	1.28	1.60	107.4**
	(4.28)	(4.27)	(4.34)	(24.84)
Long-Run Impact Effects				
Metal	−13.7[a]	2.51[a]	−4.53[a]	18.2
Embassy76	2.32	−.80	.40	4.1
Embassy85	−2.19	2.1	.54	−1.3
Libya[b]	NA	NA	NA	NA

Note: Standard errors are given in parentheses. The coefficients are unstandardized regression coefficients.
*$p \leq .10$.
**$p \leq .05$.
[a] Denotes a significant long-run impact effect or that the impact effect was significant through its effects on an important explanatory variable in the VAR analysis.
[b] All effects of Libya are temporary effects. NA Denotes not applicable.

107.4 events during the second quarter of 1986. The autoregressive nature of the system implies some persistence of this induced increase in bombings and other events. Clearly, the increase in terrorist attacks was an unintended consequence of the raid.

There appear to be only weak effects resulting from the other interventions. Neither of the embassy security measures in 1976 or 1985 had a significant influence at conventional levels. Since EMBASSY85 and EMBASSY86 occurred so closely in time, each was included in the estimations separately. Table 2 reports results using EMBASSY85; similar results hold using EMBASSY86.

Model 2

Model 2 included five series: skyjackings, assassinations, hostage, threats, and "other." This model was

FIGURE 1

Actual and Predicted Skyjackings (Model 1: 1970:1–88:4)

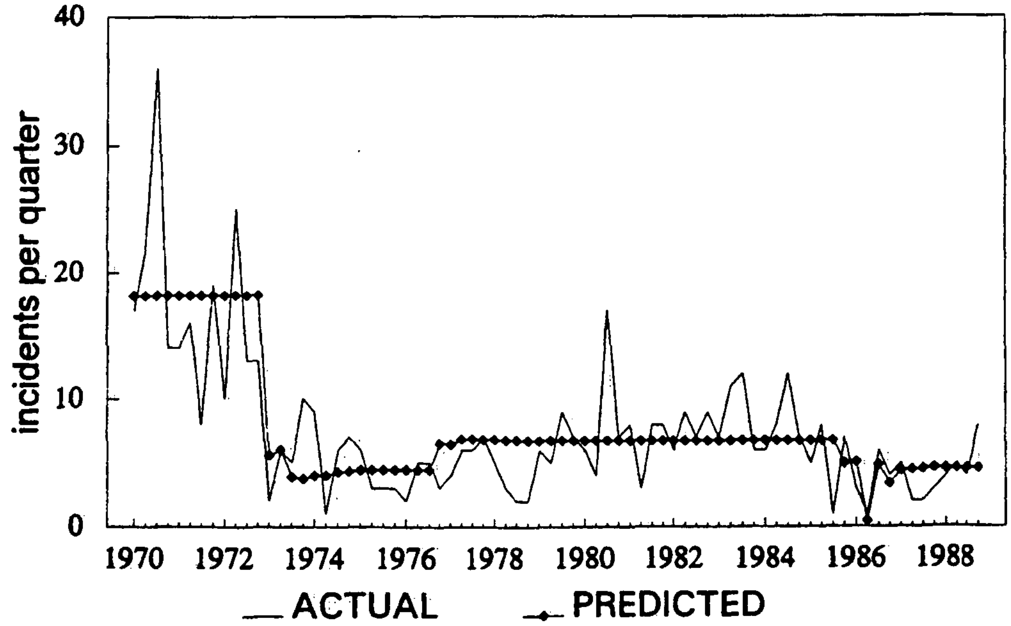
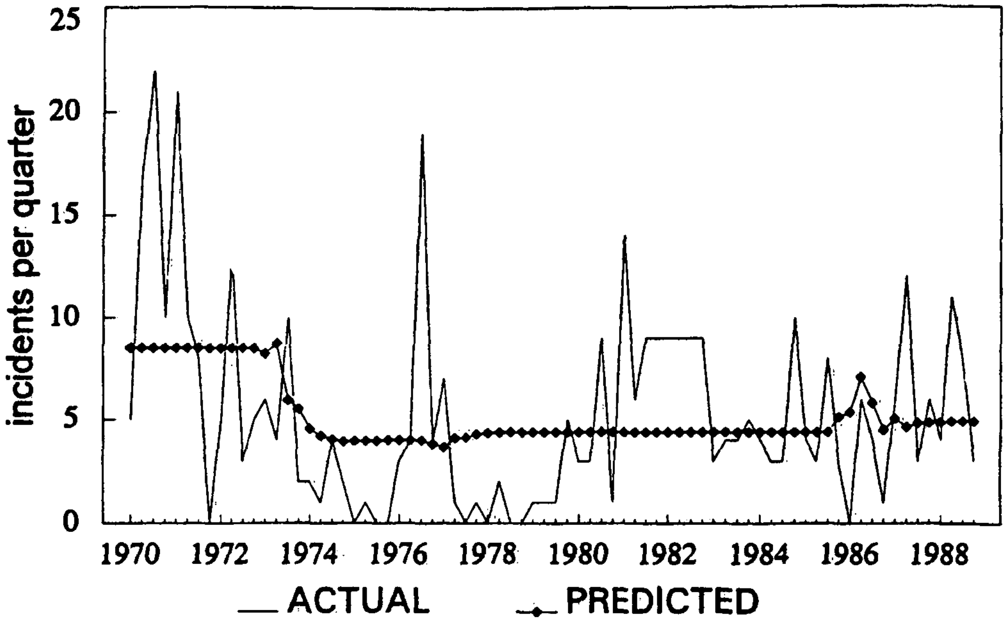


FIGURE 2

Actual and Predicted Protected-Persons Attacks (Model 1: 1970:1–88:4)



designed to consider a richer set of targets than model 1. Most important, we distinguished between non-resource-using threats and resource-using events. Although costly for governments to police, threats are relatively inexpensive incidents for terrorists to instigate. In addition, the assassination series was included to ascertain whether the hardening of targets may have led to more killings as protected individuals left more-secure grounds. This run tested the effects of the same interventions as those of model 1. As before, we estimated the model with four interventions viewing EMBASSY85 and EMBASSY86 as alternative interventions.

In contrast to model 1, the chi-squared statistic indicated that a lag length of four quarters was appropriate. The chi-squared statistic that all coefficients on lags greater than four (greater than two) were equal to zero had a significance level of .999 (.028).

FIGURE 3

Actual and Predicted Hostage Events (Model 1: 1970:1–88:4)

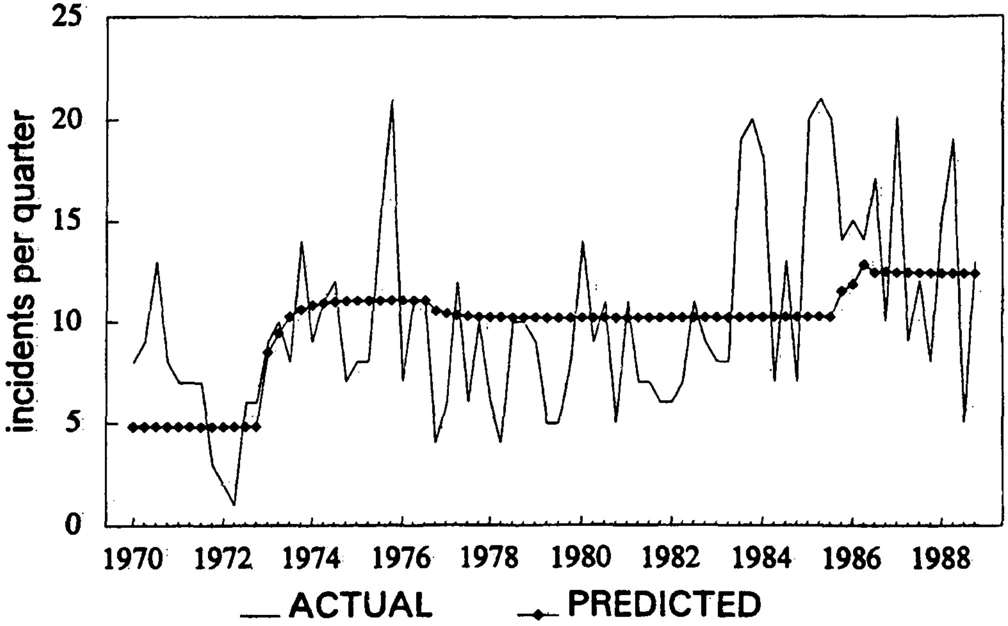


TABLE 3

Variance Decomposition for Model 2: Percentage of Forecast Error Variance at 4-, 8-, and 24-Quarter Forecast Horizons

ATTACK MODES AND FORECAST HORIZONS	ORDERING FOR TIME SERIES					REVERSE ORDERING FOR TIME SERIES				
	SKYJACKINGS	HOSTAGE	ASSASSINATIONS	THREATS	OTHER	OTHER	THREATS	ASSASSINATIONS	HOSTAGE	SKYJACKINGS
Skyjackings										
4-qu	90.7	1.38	4.67	1.56	1.71	2.29	15.9	8.44	2.28	78.4
8-qu	85.2	4.90	5.38	2.54	2.00	2.45	16.0	9.07	6.14	66.4
24-qu	84.4	5.05	5.54	2.62	2.37	2.85	16.3	9.09	6.26	65.5
	(.726)	(.305)	(.147)	(.915)	(.706)	(.706)	(.915)	(.147)	(.305)	(.725)
Hostage										
4-qu	8.62	77.5	.377	1.15	12.3	11.7	6.11	2.41	75.7	4.11
8-qu	13.9	68.7	.620	1.20	15.8	15.4	7.08	2.62	67.2	7.72
24-qu	14.1	67.9	.927	1.20	15.9	16.1	7.02	2.84	66.2	7.84
	(.279)	(.554)	(.692)	(.259)	(.242)	(.242)	(.259)	(.692)	(.554)	(.279)
Assassinations										
4-qu	6.00	8.16	78.5	.502	6.84	12.9	5.51	74.1	7.18	1.67
8-qu	13.1	6.85	61.0	.564	18.5	27.0	10.4	52.4	6.39	3.77
24-qu	14.1	6.87	59.8	.761	18.4	27.2	10.5	51.5	6.57	4.15
	(.809)	(.235)	(.006)	(.159)	(.207)	(.207)	(.159)	(.006)	(.235)	(.809)
Threats										
4-qu	30.4	8.93	10.1	42.9	8.36	14.6	65.0	5.37	7.65	7.41
8-qu	34.4	7.97	11.4	35.7	10.5	17.4	57.1	7.71	8.93	8.82
24-qu	35.8	8.05	11.3	34.7	10.2	16.8	56.5	8.35	9.12	9.20
	(.125)	(.049)	(.030)	(.000)	(.170)	(.170)	(.000)	(.030)	(.049)	(.125)
Other										
4-qu	2.74	2.38	10.5	9.08	75.2	91.5	4.91	1.29	1.56	.702
8-qu	4.97	3.34	10.5	9.33	71.9	86.8	7.24	1.99	2.76	1.15
24-qu	6.84	3.39	10.7	9.47	69.6	84.3	8.41	2.69	2.94	1.70
	(.823)	(.731)	(.804)	(.277)	(.211)	(.211)	(.277)	(.804)	(.731)	(.823)

Note: The numbers in parentheses are the significance levels for the joint hypothesis that all lagged coefficients of the variable in question can be set equal to zero. Quarters are denoted by "qu." The underlying data are ITERATE 2 and 3 and are quarterly observations from 1968:1 to 1988:4.

The variance decompositions for model 2 are reported in Table 3. The left-hand half shows the decomposition using the ordering skyjackings → hostage → assassinations → threat → other, and the right-hand half the reversed ordering. Although each incident type explains a large portion of its own forecast error variance, there is a complex set of interactions between series. The threats series is associated with all of the other series: depending on the ordering, the other four series explain as much as 65% of the forecast error variance of threat. The ordering is particularly important for the skyjackings and threat series; the correlation coefficient between the residuals of the two series in the first ordering is .382, implying that these two series move strongly together.

The information obtained from the variance decompositions was used to impose the zero restrictions and to test the impacts of the interventions. Specifically, each equation contained a constant, the three seasonal dummies, four interventions, and its own lags 1–4. The skyjackings equation also contained four lags of threats, and the threats series contained four lags of all of the other series. Using Granger-causality test results, the skyjackings and other series were allowed to affect the hostage and assassination series. Also, we allowed the other series to be affected by assassinations. The richness of the interactions is such that an intervention affecting assassinations may ultimately affect all of the other incident types.

As shown in Table 4, the results of model 2 yield several new insights. Much of the increased terrorism generated by the Libyan raid augmented non-resource-using threats: nearly half of the terrorist reaction took the form of words, not deeds. This is not to say that threats are not disruptive, since citizens and governments may feel obliged to commit real resources in response to a threat or hoax. Tourists may alter travel plans, involving sizable revenue losses for a nation's tourist industry, and firms may alter plans for direct foreign investment. Terrorists lashed back at the raid with relatively low-cost incidents (threats and bombings) in the hopes of imposing sizable costs on others. The estimated impact and long-run effects of metal detectors on skyjackings is nearly that of model 1. However, we now find a larger substitution into other hostage events and assassinations. Metal

TABLE 4

Coefficients and Signficance Levels of the Impact Effects of Model 2

ANTITERRORISM POLICIES	SKYJACKINGS	HOSTAGE	ASSASSINATIONS	THREATS	OTHER
Temporary Effects					
Metal	−14.1**	11.6**	6.58*	1.75	11.3
	(2.52)	(3.10)	(2.96)	(3.03)	(9.74)
Embassy76	2.51*	−1.41	3.56*	8.67**	−18.6*
	(1.37)	(1.26)	(1.89)	(1.98)	(9.81)
Embassy85	.100	3.54*	−.967	−5.31*	−2.81
	(2.44)	(1.57)	(1.438)	(2.51)	(6.88)
Libya	−4.83	−1.62	1.57	50.5**	58.4*
	(4.56)	(4.20)	(4.39)	(4.78)	(22.51)
Long-Run Impact Effects					
Metal	−13.0[a]	5.3[a]	4.1[a]	−9.5[a]	17.8[a]
Embassy76	.98[a]	−.20[a]	8.2[a]	11.8[a]	−21.1[a]
Embassy85	−.52[a]	5.4[a]	−1.7	4.1[a]	−5.4
Libya[b]	NA	NA	NA	NA	NA

Note: Standard errors are in parentheses. The coefficients are unstandardized regression coefficients.
*$p \le .10$.
**$p \le .05$.
[a] Denotes a significent long-run impact effect or that the impact effect was significant through its effects on an important explanatory variable in the VAR analysis.
[b] All effects of LIBYA are temporary effects.

detectors did not have a negative impact on the hostage series, since this series included mostly kidnappings and very few barricade missions. The long-run effect on threats was estimated to decline by over nine incidents per quarter. Since many threats involved planes and embassies, which were made more secure by metal detectors, these threats may have declined as they became less credible: terrorists may have substituted deeds for words owing to metal detectors.

Model 2 is noteworthy, since the two embassy fortifications have significant impact effects. EMBASSY76 has the impact of increasing threats by over eight incidents per quarter. There is only weak evidence that EMBASSY76 affected the other series: at the .10 level for a one-tailed test, EMBASSY76 increased skyjackings and assassinations, while it decreased other events. The impact and long-run effect of EMBASSY76 on assassinations are especially interesting, since they suggest that protected individuals were more prone to assassinations when leaving secured grounds. Since threats require few inputs, these events can increase without necessitating substitution out of other events. EMBASSY85 is estimated to have increased hostage events at the expense of threats. After the late 1970s, hostage events consisted *almost entirely* of kidnappings; hence, it is not surprising that EMBASSY85 led to substitution into hostage events. On impact, hostage events rose by 3.54 incidents per quarter. In the long run, hostage events gradually increased to 5.4 additional incidents per quarter.

The predicted functions depicted in Figures 4–5 show the dynamic impacts of all of the interventions. For brevity, we reproduce the predicted functions for assassinations and threats only. In Figure 4, metal detectors in 1973 caused a rise in assassinations that was large and then permanently reinforced by EMBASSY76. In Figure 5, metal detectors induced a long-run decline in threats, while EMBASSY76 led to a short-run and long-run increase in threats. The Libyan raid in 1986 created a sharp rise in threats that declined over the next two years. Metal detectors, EMBASSY76, and the Libyan raid had some very costly unintended consequences.

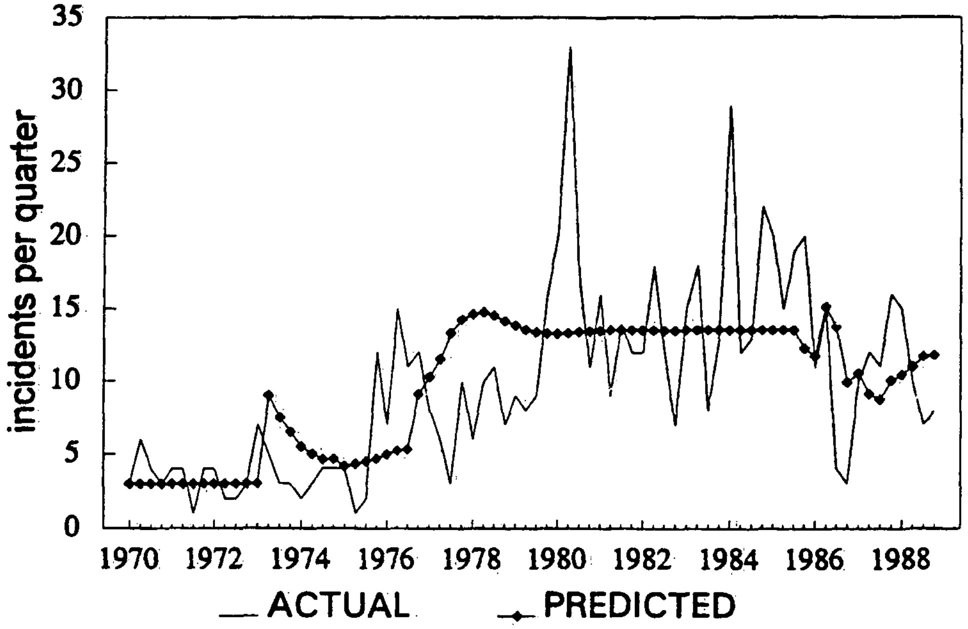
Model 3

Model 3 was designed to focus on the United States. Three series were included: attacks against protected persons, attacks against U.S. and British interests not involving protected persons *(US, UK attacks),* and other attacks. The protected-persons series is more inclusive than that of model 1, since hostage missions were not purged. American and British attacks were combined because these countries cooperated in the Libyan raid. Moreover, both nations have maintained similar stances toward terrorism. On 17 July 1986, the U.S. Senate ratified, by a vote of 87 to 10, an extradition treaty with Britain, making it easier for British authorities to obtain custody of Irish Republican Army members seeking political asylum in the United States (Mickolus, Sandler, and Murdock 1989, vol. 2, 430). Such treaties are unusual. Five policy impacts were tested in model 3: METAL, EMBASSY76, EMBASSY86, REAGAN LAWS, and LIBYA.

Unlike the other models, one of the series in model 3 was found to be nonstationary. Using undifferenced data, the predicted function for the other series appeared nearly linear, and the sum of the coeffi-

FIGURE 4

Actual and Predicted Assassinations (Model 2: 1970:1–88:4)



cients on "own" lags was nearly unity. With an augmented Dickey–Fuller test, we were not able to reject a null of nonstationarity. We reestimated the equations using the first-difference of the other series (called *other differenced*). Based on the mean-square error, we selected the specification in which the interventions affected the level, not the slope, of the other events series.

The discussion refers to the estimation using first-differences of the other event series. A lag length of two quarters was selected, because the chi-squared statistics for lag lengths of four and two quarters were significant at the .215 and .815 levels, respectively. Decompositions are reported for the ordering consisting of *protected persons→US, UK attacks→other* and for its reverse ordering. In Table 5, attacks against the United States and the British explain about 77% of their own forecast error variance. It is particularly

FIGURE 5

Actual and Predicted Threats (Model 2: 1970:1–88:4)

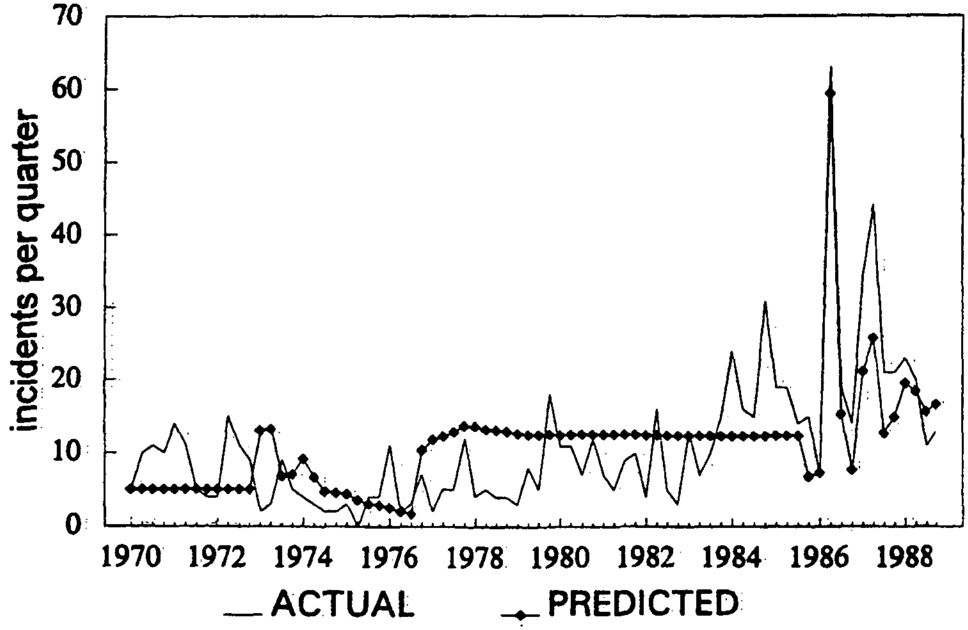


TABLE 5

Variance Decomposition for Model 3: Percentage of Forecast Error Variance 4-, 8-, and 24-Quarter Forecast Horizons

ATTACK MODES AND FORECAST HORIZONS	ORDERING FOR TIME SERIES			REVERSE ORDERING FOR TIME SERIES		
	PROTECTED PERSONS	US, UK ATTACKS	OTHER (DIFFERENCED)	OTHER (DIFFERENCED)	US, UK ATTACKS	PROTECTED PERSONS
Protected Persons						
4-qu	97.8	1.56	.681	.988	5.67	93.3
8-qu	97.7	1.61	.667	.969	5.55	93.5
24-qu	97.7	1.62	.667	.969	5.55	93.5
	(.000)	(.564)	(.583)	(.583)	(.564)	(.000)
US, UK Attacks						
4-qu	20.2	77.1	2.75	6.05	77.7	16.3
8-qu	22.1	75.2	2.69	6.01	77.1	16.9
24-qu	22.1	75.2	2.69	6.00	77.0	17.0
	(.004)	(.342)	(.130)	(.130)	(.342)	(.004)
Other (Differenced)						
4-qu	1.70	8.57	89.7	95.7	1.34	2.93
8-qu	1.70	8.57	89.7	95.7	1.34	2.93
24-qu	1.70	8.57	89.7	95.7	1.34	2.93
	(.279)	(.248)	(.266)	(.266)	(.248)	(.279)

Note: The numbers in parentheses are the significance levels for the joint hypothesis that all lagged coefficients of the variable in question can be set equal to zero. Quarters are denoted by "qu." The underlying data are from ITERATE 2 and 3 and are quarterly observations from 1968:1 to 1988:4.

noteworthy that regardless of the ordering, protected-persons attacks explain a sizable proportion of the forecast error variance of *US, UK attacks.* Since the protected-persons series included numerous attacks against U.S. and British protected persons, the interrelationship between the two series is not unexpected. Moreover, the protected-persons series Granger-causes *US, UK attacks* at the .004 level; the correlation coefficient between the residuals in the protected-persons and *US, UK attacks* equations is .269.

Using the decompositions to impose the zero-restrictions, we reestimated each equation with SUR. Each equation in the reestimation contained a constant, three seasonal dummy variables, the five interventions, and two "own-lags." The other (differenced) series used the first-differences of its own lags and the first-differences of the five interventions. Granger-causality tests indicated that the *US, UK attacks* equation contained two lags of the protected-person series.

The impacts of the five policies are reported in Table 6. In model 3, metal detectors did not appear to affect the total number of incidents directed at the United States and the United Kingdom. Given the previous significant effects of metal detectors, it is apparent that there were substitutions between the types of incidents directed at the two nations. Metal detectors did have a significant negative impact on the first-difference of other events not involving either protected persons or U.S. and British interests. The most important result from this model concerns the impact of EMBASSY76. By disaggregating attacks into those specifically directed toward the United

TABLE 6

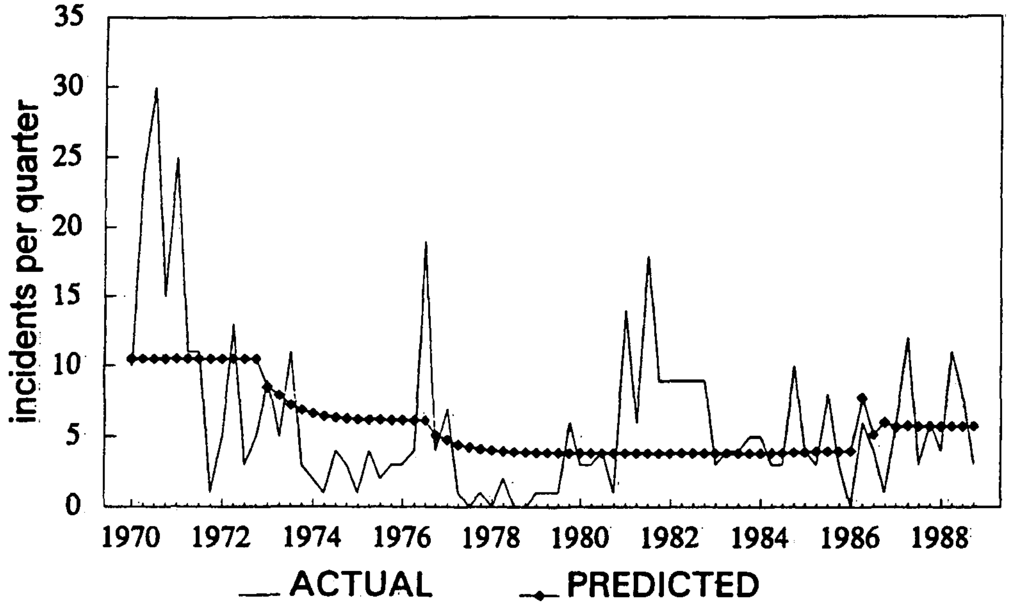
Coefficients and Significance Levels of the Impact Effects of Model 3

ANTI-TERRORISM POLICIES	PROTECTED PERSONS	US, UK ATTACKS	OTHER (DIFFERENCED)[a]
Temporary Effects			
Metal	−2.09	3.83	−99.8**
	(1.80)	(4.49)	(23.01)
Embassy76	−.934	−12.8**	−63.4**
	(1.45)	(3.90)	(20.77)
Reagan laws	.124	−.095	−15.8
	(1.93)	(4.63)	(20.97)
Embassy86	.897	−5.13	−22.5
	(2.34)	(5.67)	(21.49)
Libya	3.37	62.0**	29.8*
	(5.08)	(12.20)	(15.40)
Long-Run Impact Effects			
Metal	−3.3	.10	-73^{b}
Embassy76	2.3	-15.7^{b}	-32^{b}
Reagan laws	.13	−.20	−7
Embassy86	1.89	−3.8	−11
Libya[c]	NA	NA	NA

Note: Standard errors are given in parentheses. The coefficients are unstandardized regression coefficients.
$^{*}p \le .10$.
$^{**}p \le .05$.
[a] All impact effects are permanent effects. The impacts shown are changes in the level of "other."
[b] Denotes a significant long-run impact effect or that the impact effect was significant through its effects on an important explanatory variable in the VAR analysis.
[c] All effects of Libya are temporary effects.

FIGURE 6

Actual and Predicted Protected Persons Attacks (Model 3: 1970:1–88:4)



Note: Data includes those hostage events and skyjackings involving protected persons.

States and Britain, we find that EMBASSY76 played an important role. Inasmuch as EMBASSY76 involved fortifying U.S. embassies, this disaggregation displays the impact of EMBASSY76 better than the first two runs. Previous studies (e.g., Cauley and Im 1988) never disaggregated attacks and had disappointing findings. On impact, EMBASSY76 reduced *US, UK attacks* by nearly 13 incidents per quarter; in the long run, EMBASSY76 reduced these attacks by nearly 16 incidents per quarter. This model, however, could not show the substitution into other kinds of attacks like assassinations.

The Libyan raid increased attacks on both U.S. and British interests and on other nations, but two-thirds of the increase was directed at the two nations participating directly or indirectly in the raid. Neither EMBASSY86 nor REAGAN LAWS had any significant impact. Apparently U.S. domestic laws geared to deter transnational terrorism did not work as intended. In Figure 6, the predicted function for protected persons appears to decline gradually from 1973 to the late 1970s. Both metal detectors and EMBASSY76 added to this decline, while the Libyan raid reversed the trend.

CONCLUSION

If governments are to succeed in the efficient allocation of resources among competing means to thwart transnational terrorism, then governments must be able to calculate the impact of alternative passive and active responses. We put forth a methodology for evaluating antiterrorism policies. Our procedure combines VAR modeling with intervention analysis and identifies each policy's quarterly impact, while accounting for the significant interrelationships among the time series for each model. These interrelationships were ignored by previous studies that examined the impact of one or more policies on a *single* time series at a time (Brophy-Baermann and Conybeare n.d.; Cauley and Im 1988; Landes 1978). In consequence, the earlier methodologies worked best in instances where little or no interaction characterized the series. The VAR–intervention procedure is best for forecasting future terrorist trends and campaigns. In particular, the technique can identify terrorist tactics that are either substitutes or complements for one another.

The following conclusions are drawn from our study:

1. The Libyan raid caused a number of different terrorist tactics to increase. A small portion of this increase spilled over to nations not involved in the retaliatory strike. Over half of the temporary increase in terrorism took the form of non-resource-using incidents.
2. Fortification of U.S. embassies and missions in 1976 reduced attacks against U.S. interests but resulted in a substitution into assassinations.
3. Metal detectors decreased skyjackings and threats but increased assassinations and other kinds of hostage incidents not protected by the detectors.
4. If governments are to evaluate a policy correctly, substitutions and complementarities must be accounted for, along with indirect effects resulting from interactions between series. For example, the

benefits from metal detectors cannot be adequately measured by the value placed on a reduced number of skyjackings, since other events (e.g., crimes against protected persons) were affected indirectly.

5. The identification of substitutes and complements is sensitive to VAR interactions.
6. In this study, all series showed some interrelationship. The finer the disaggregation, the greater the interaction.

A number of policy insights can be drawn. First, the unintended consequences of an antiterrorism policy may be far more costly than intended consequences, and must be anticipated. In the case of metal detectors, kidnappings increased; in the case of embassy fortification, assassinations became more frequent. Protected persons may have faced more life-threatening attacks owing to security measures. By cutting down on threats, metal detectors may have induced terrorists to substitute deeds for words. Second, piecemeal policy, in which a single attack mode is considered when designing antiterrorism action, is inadequate. This follows because the various attack modes are interrelated through substitutability and complementarity. Third, the retaliatory raid on Libya appeared to increase terrorism in the near term, but did not have a significant long-run impact, good or bad. Fourth, U.S. domestic laws, instituted in 1984 to thwart terrorism, did not curb terrorist attacks against U.S. interests.

The methodology used here can be fruitfully employed elsewhere. In particular, it can assess the impact of significant terrorist events (e.g., the downing of Pan Am Flight 103 in December 1988) on tourism.

Notes

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1. *State sponsorship of terrorism occurs when nations* contract with terrorist groups to perform acts that impose costs on countries perceived as enemies by the sponsor and surrogate (Mickolus 1989).

2. Other econometric studies have investigated terrorist success in hostage-taking missions (Sandler and Scott 1987), the underlying factors behind the length of hostage negotiations (Atkinson, Sandler, and Tschirhart 1987), and cycles in terrorist events (Im, Cauley, and Sandler 1987). Empirical studies have also investigated whether terrorist incidents encourage or discourage further acts through a demonstration effect that gives rise to imitation diffusion (Hamilton and Hamilton 1983; Midlarsky, Crenshaw, and Yoshida 1980). Poe (1988) used elementary regression techniques to demonstrate the ineffectiveness of hardline policies. A more complete list of references can be found in Lichbach 1992.

3. A household production approach is assumed (see Ehrlich 1973).

4. These choice-theoretical problems are modeled in Islam and Shahin 1989 and Sandler, Tschirhart, and Cauley 1983. The reader should consult these articles for an explicit theoretical model.

5. Many of the ideas presented in this section are discussed in Granger and Newbold 1986, chap. 7–8. Doan 1988, chap. 8 is particularly useful for the implementation of a VAR analysis.

6. Formally, for each incident type j in equation system 3, we obtained the vector moving average (VMA) representation:

$$y_j(t) = c + \sum_{i=1}^{3} d_i D_i(t) + \sum_{i=1}^{n} \sum_{\tau=1}^{\infty} \Theta_{ji\tau} \varepsilon_i(t-\tau) + \sum_{i=1}^{K} \sum_{\tau=0}^{\infty} \gamma_{ji\tau} p_i(t-\tau) + \varepsilon_j(t),$$

where $D_i(t)$ is the value of each seasonal dummy in period t, while c, d_i, $\Theta_{ji\tau}$, and $\gamma_{ji\tau}$ are all parameters. Since we cannot estimate the VMA directly, we used the residuals of equation system 3 and the coefficients from the inverted VAR model. We decomposed the variance of each incident type j into the percentages attributable to innovations in the other incident types, as well as the "own" innovation. Since the residuals in equation system 3 can be correlated across equations, we used the orthogonalized innovations obtained from a Choleski decomposition. Doan (1988) contains an excellent discussion of Choleski decomposition.

7. In terms of equations 1 and 2, if $\varepsilon_1(t)$ and $\epsilon_2(t)$ are correlated, there are two possible ways to obtain "pure" shocks to each incident type. First, $\epsilon_1(t)$ can be viewed a pure shock to incident type 1. This shock affects incident type 1 directly *and* $y_2(t)$ through the correlation between $\epsilon_1(t)$ and $\epsilon_2(t)$. The remainder of $\epsilon_2(t)$ is deemed a "pure" shock to incident type 2. Alternatively, $\epsilon_2(t)$ can be viewed a pure shock to incident type 2. This shock affects incident type 2 directly *and* $y_1(t)$ through the correlation between $\epsilon_1(t)$ and $\epsilon_2(t)$. The remainder of $\epsilon_1(t)$ is deemed a "pure" shock to incident type 1. The first ordering of this Choleski decomposition implies $\epsilon_1(t) \rightarrow \epsilon_2(t)$ while the second implies $\epsilon_2(t) \rightarrow \epsilon_1(t)$.

When the innovations in the various series are contemporaneously correlated, the ordering of the variables used in the factorization can be important for the variance decomposition. In a VAR analysis, at the 5% level, a statistically significant correlation coefficient is usually taken to be about .2. In all models estimated, we placed correlated series adjacent to each other; reversing the ordering allows us to determine the importance of the alternative assumptions.

8. Denote the deseasonalized value of $y_j(t)$ by $\hat{y}_j(t)$. Formally, we set all of the innovations equal to zero and plot time path:

$$\hat{y}_j(t) = c + \sum_{i=1}^{K} \sum_{\tau=0}^{\infty} \gamma_{ji\tau} p_i(t-\tau).$$

9. Specifically, we estimated the equation

$$\Delta y_i(t) = r_0 + \rho y_i(t-1) + \sum_{j=1}^{4} r_j \Delta y_i(t-j),$$

for each series y_i. A null nonstationarity can be rejected if the estimated value of r_0 is not significantly different from zero and ρ is significantly less than zero, but greater than minus two. Tables compiled by Fuller (1976) contain the relevant test statistics. If a variable needs to be differenced, it is not clear whether the effects of the intervention on that variable also need to be differenced. For example, ignoring the constant and seasonals, two of the possible presentations of y_i are

$$\Delta y_i(t) = \sum_{k} a_{ik} p_k(t) + \sum_{\ell} \sum_{j} b_{\ell ij} \Delta y_i(t-\ell)$$

$$\Delta y_i(t) = \sum_{k} a_{ik} \Delta p_k(t) + \sum_{\ell} \sum_{j} b_{\ell ij} \Delta y_i(t-\ell).$$

In the first equation, the interventions are assumed to have a permanent effect on the slope of incident type y_i. In the second specification, the interventions are assumed to have a permanent effect on the level. Imposing the first specification is straightforward; each value of $y_i(t)$ is replaced by its first difference in equation system 3. To impose the second specification, a near-VAR must be estimated. We estimated the system using both specifications. In model 3, the first specification yielded the best mean-square error for the "other-differenced" series.

10. ITERATE 2 is available through the Inter-University Consortium for Political and Social Research, University of Michigan, Ann Arbor. ITERATE 3 is available from Vinyard Software, 2243 Beacon Lane, Falls Church, VA 22043-1709. The working definition of *transnational terrorism* employed by ITERATE is the use—or threat of use—of anxiety-inducing, extranormal violence for political purposes by any individual or group, whether acting for or in opposition to established governmental authority, when such action is intended to influence the attitudes and behavior of a target group wider than the immediate victims and when, through the nature of its institutional or human victims, or through the mechanics of its resolution, its ramifications transcend national boundaries (Mickolus, Sandler, and Murdock 1989, xii).

11. See n. 10.

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CONCEPTUAL "STRETCHING" REVISITED: ADAPTING CATEGORIES IN COMPARATIVE ANALYSIS

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When scholars extend their models and hypotheses to encompass additional cases, they commonly need to adapt their analytic categories to fit the new contexts. Giovanni Sartori's work on conceptual "traveling" and conceptual "stretching" provides helpful guidance in addressing this fundamental task of comparative analysis. Yet Sartori's framework draws upon what may be called classical categorization, which views the relation among categories in terms of a taxonomic hierarchy, with each category having clear boundaries and defining properties shared by all members. We examine the challenge to this framework presented by two types of nonclassical categories: family resemblances and radial categories. With such categories, the overly strict application of a classical framework can lead to abandoning to category prematurely or to modifying it inappropriately. We discuss solutions to these problems, using examples of how scholars have adapted their categories in comparative research on democracy and authoritarianism.

Stable concepts and a shared understanding of categories[1] are routinely viewed as a foundation of any research community. Yet ambiguity, confusion, and disputes about categories are common in the social sciences. A major source of this difficulty is the perpetual quest for generalization. As scholars seek to apply their models and hypotheses to more cases in the effort to achieve broader knowledge, they must often adapt their categories to fit new contexts. One of the most incisive treatments of this problem of adapting categories is Giovanni Sartori's (1970, 1984) work on conceptual *traveling* (the application of concepts to new cases) and conceptual *stretching* (the distortion that occurs when a concept does not fit the new cases).

This is an old debate, and it might appear that this problem of categorization has been superseded by new analytic and statistical approaches. However, this is not the case. Scholars accustomed to the language of "variables" will recognize that issues raised here are closely related to problems of establishing the validity of observation and measurement across cases. For example, analysts who have carefully derived and tested a set of hypotheses about political participation in one set of cases will commonly wish to probe the generality of their findings by examining the same hypotheses in additional cases. To do so, they must first establish that political participation has a sufficiently similar meaning in the new cases. An excessive concern with the difficulties of establishing equivalence among contexts of analysis could, of course, lead to the abandonment of the comparative enterprise altogether. The merit of Sartori's approach is that it encourages the scholar to be attentive to context, but *without* abandoning broad comparison.

In recent years, new interest in the problem of applying categories across diverse contexts has been generated by the rise of a school of comparative-historical analysis,[2] as well as by the comparative politics literature on authoritarianism and corporatism in the 1970s and on democratization in the 1980s and 1990s.[3] It is evident from these bodies of scholarship that broad comparison requires a use of categories that is sensitive to context. Further, the historical depth in many of these studies offers a useful reminder that the problem of conceptual stretching can arise not only from movement across cases but also from change over time within cases. Hence, the challenge of achieving the virtue of conceptual traveling without committing the vice of conceptual stretching remains very much with us today.

We shall examine how categories change—or should change—as they are applied to new cases. Sartori's original framework is based on the assumptions of what is sometimes called classical categorization, in which the relation among categories is understood in terms of a taxonomic hierarchy of successively more general categories (1970, 1038). Each category possesses clear boundaries and defining properties that are shared by all members and that serve to locate it in the hierarchy. Yet twentieth-century linguistic philosophy and contemporary cognitive science have presented a fundamental challenge to this understanding of categories by claiming that many types of categories do not possess these attributes (Lakoff 1987). This challenge might seem to undermine Sartori's approach. However, we show that these alternative types of categories can be treated in a way that is distinct from, yet complementary to, Sartori's perspective.

To provide a base line against which alternative perspectives on categories can be evaluated, we first review Sartori's procedure for modifying categories. We then explore the distinctive problems that arise in dealing with types of categories that do not fit the classical pattern, which is the basis of Sartori's approach. First, we examine the issues that arise with "family resemblance" categories. This discussion suggests that Sartori's procedure can be applied too strictly, causing analysts to abandon a category prematurely when it initially does not appear to fit additional cases. We then consider what cognitive scientists call the "radial" category,[4] providing a rationale for why different types of categories (e.g., democracy, as opposed to authoritarianism) are modified in distinct ways as they are adapted to fit new cases. We conclude by suggesting new guidelines for adapting categories in the process of comparative analysis.

AVOIDING CONCEPTUAL STRETCHING: SARTORI'S FRAMEWORK

A central element in the classical view of categories, which provides the underpinning for Sartori's framework, is the understanding of *extension* and *intension* (Sartori 1970, 1041; idem. 1984, 24). The extension of a category is the set of entities in the world to which it refers. The intension is the set of meanings or attributes that define the category and determine membership.

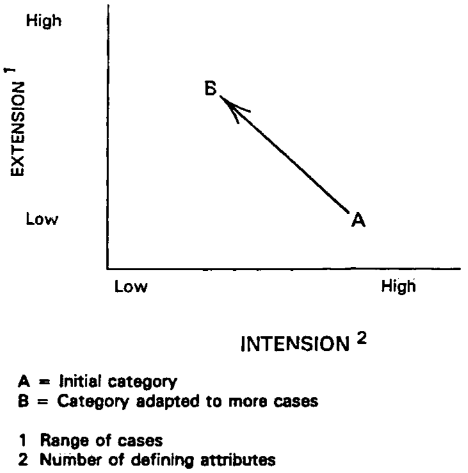
Two complementary patterns in the relation between extension and intension are of concern here, namely, the occurrence of (1) more specific categories with more limited extension and greater intension and (2) more general categories with greater extension and more limited intension. Some philosophers have held that these patterns reflect a "law of inverse variation" (Angeles 1981, 141). In a taxonomic hierarchy, these more specific and more general categories occupy subordinate and superordinate positions, with the extension of the subordinate categories contained inside the superordinate ones. The hierarchy represented by these sets of terms can be called, adapting Sartori's label, a "ladder of generality."[5]

An example will serve to illustrate these patterns. In Max Weber's famous typology, patrimonial authority is a type of traditional authority, which is one of his three overall types of authority or legitimate domination, which, in turn, is one type within the broader category of domination (1978, 212–15, 226, 231). Within each successive pair of categories, the first is subordinate, the second, superordinate. In relation to each subordinate category, the corresponding superordinate category contains a less specific meaning and covers more cases; thus, it has greater extension and less intension.

This classical understanding of categories helps address the problem of conceptual stretching. When scholars take a category developed for one set of cases and extend it to additional cases, the new cases may be sufficiently different that the category is no longer appropriate in its original form. If this problem arises, they may adapt the category by climbing the ladder of generality, thereby obeying the law of inverse variation. As they increase the extension, they reduce the intension to the degree necessary to fit the new contexts. For example, scholars engaged in a comparative study of patrimonial authority might add cases to their analysis that only marginally fit this category. To avoid conceptual stretching, they might move up the ladder of generality and refer to the larger set of cases as instances of traditional authority. This interplay between extension and intension on the ladder of generality is illustrated in Figure 1. With the categories obeying the law of inverse variation, the ladder of generality appears as a line of negative slope.[6]

FIGURE 1

The Ladder of Generality



In short, this framework helps researchers proceed with greater care when addressing one of the basic tasks of comparative research: the effort to achieve broader knowledge through analyzing a wider range of cases. The value of this framework merits emphasis particularly in light of skepticism, on the part of scholars committed to an "interpretive" perspective, about the possibility of achieving a viable comparative social science (Geertz 1973, 1983; MacIntyre 1971; Rabinow and Sullivan 1987; Taylor 1971; Winch 1959). Sartori's framework addresses some of the important concerns raised by this perspective, namely, that broad comparison is difficult, that political and social reality is heterogeneous, that applying a category in a given context requires detailed knowledge of that context, and that it is easy to misapply categories. The ladder of generality offers a specific procedure to address these issues. This procedure has deservedly served as a benchmark for analysts who wrestle with the problem of extending categories to new cases.

FAMILY RESEMBLANCE CATEGORIES

The application of the ladder of generality assumes the clear boundaries and defining attributes of classical categories. An exploration of family resemblance categories shows that at times, this assumption

FIGURE 2

An Iterated Generalization: The Case of Family Resemblance

Cases	True Distribution of Attributes*						Cases Included in Generalization (Extension)	Attributes Included in Generalization (Intension)
A	1	2	3	4	5		A	1 2 3 4 5
B	1	2	3	4		6	A B	1 2 3 4
C	1	2	3		5	6	A B C	1 2 3
D	1	2		4	5	6	A B C D	1 2
E	1		3	4	5	6	A B C D E	1
F		2	3	4	5	6	A B C D E F	

* All cases have five of the six attributes, and each of the six cases is missing a different attribute.

should be relaxed. Ludwig Wittgenstein's idea of family resemblance entails a principle of category membership different from that of classical categories, in that there may be no single attribute that category members all share. The label for this type of category derives from the fact that we can recognize the members of a human genetic family by observing attributes that they share to varying degrees, as contrasted to nonfamily members who may share few of them. The commonalities are quite evident, even though there may be no trait that all family members, as family members, have in common (Wittgenstein 1968, nos. 65–75; see also Canfield 1986 and Hallett 1977, 140–41, 147–48).[7]

A similar pattern often appears in the social sciences. A category, defined in a particular way, may fit a number of cases reasonably well, but on close examination it can become clear that for most cases the fit is not perfect. Nonetheless, the category captures a set of commonalities considered by the researcher to be analytically important. This pattern is found, for example, in the literature on corporatism, which generally presents a series of defining attributes, usually without the expectation that the full set of attributes would be found in every instance (Malloy 1977; Schmitter 1974). Thus, over many decades during the twentieth century, it was reasonable to characterize labor relations in Argentina, Brazil, Chile, and Mexico as corporative, despite variation in the features of corporative structuring, subsidy, and control of groups found in the four cases (Collier and Collier 1991).

What would happen if we applied Sartori's method to a family resemblance category? Let us consider a hypothetical exercise in comparative analysis. Suppose that (1) the analyst begins with a case study yielding a new category of theoretical interest initially appearing to have five defining attributes, (2) the initial case is one of six cases that share a family resemblance, (3) this family resemblance turns out to entail six shared attributes, and (4) each case possesses a different combination of only five of these. No attribute is shared by all cases.

Using this example (see Figure 2), we will examine the consequences if the analyst were to rigidly apply the ladder of generality. If the original case-study research were done on Case A, the intension of the initial category would encompass Attributes 1–5. Upon adding Case B to the analysis, other analysts might note that Attribute 5 was lacking. They could seek to avoid conceptual stretching by climbing the ladder of generality to a category that encompassed both cases (A and B) and whose intension was reduced to Attributes 1–4. Adding Case C could lead to a further step up the ladder to a still more general category that encompassed only Attributes 1–3. As can be seen in the figure, when this iterated process finally reached Case F, the final step up the ladder of generality would bring the elimination of the final trait, leaving a category with no attributes. Thus, the analyst might abandon the category prematurely. The example in Figure 2 serves as a warning that, in the course of applying a category to additional cases, it can be counterproductive to insist on eliminating those attributes not held in common by all the cases under consideration.

One way of avoiding this problem is to look at the larger set of cases simultaneously, so that the commonalities evident in Figure 2 would be recognized. Yet because every case is missing at least one attribute, a researcher accustomed to thinking in terms of classical categories might still conclude that this is a weak category that should be abandoned. A possible response would be to emphasize that the category is an analytic construct which the researcher should not expect to be a perfect description of each case. A well-known example of this kind of construct is the ideal type, of which each specific case is expected to be only a partial approximation.[8]

Some of the creative approaches to the refinement of categories in the field of comparative politics can be seen as attempts to deal with family resemblance.

Przeworski and Teune argue that in comparative research, conceptualization and measurement at times require a "system-specific" approach. They suggest, in effect, that in diverse contexts *different* attributes can be used as defining properties of the *same* category.[9] Nie, Powell, and Prewitt employ a similar perspective in comparing political participation in the United States and four other countries (1969, 377). For all the countries, their analysis focuses on four relatively standard attributes of participation. However, in analyzing a fifth attribute—membership in a political party—they observe that whereas in four of the countries it has a roughly equivalent meaning, party membership in the United States has a significantly different form and meaning. The authors conclude that in the United States, involvement in electoral campaigns reflects an equivalent form of political participation. Hence, for that country, they analyze campaign participation instead of party membership.

As in this last example, it is evident that family resemblance can sometimes be assessed by identifying attributes that are present to varying degrees in particular cases, rather than being simply present or absent. This can be accomplished by applying some form of multidimensional scaling that specifies underlying dimensions for comparing cases. Yet it is important to remember that multidimensional scaling does not eliminate the original problem of forming the concept. In the spirit of Sartori's dictum "concept formation stands prior to quantification" (1970, 1038), one must recognize that a prerequisite for such scaling is to establish what it is that is being scaled.[10]

When the analyst encounters a family resemblance pattern, two priorities must be addressed. First, in assessing the attributes empirically, one must avoid an application of the ladder of generality that is so strict as to result in the inappropriate rejection of a potentially useful category. Second, it is essential to explore the underlying analytic relationship among the attributes that constitute the family resemblance, thereby establishing the justification for retaining the category. A concern with this analytic relationship is central to the discussion of radial categories, to which we now turn.

RADIAL CATEGORIES

Another type of category that does not fit the classical pattern is the radial category, analyzed by cognitive scientists such as Lakoff (1987, chap. 6). As with family resemblance, with radial categories it is possible that two members of the category will not share all of what may be seen as the defining attributes. In contrast to the family resemblance pattern, with radial categories the overall meaning of a category is anchored in a "central subcategory," which corresponds to the "best" case, or prototype, of the category.[11] In the process of cognition, the central subcategory functions as a gestalt, in that it is constituted by a bundle of traits that are learned together, understood together, and most quickly recognized when found together. "Noncentral subcategories" are variants of the central one. They do not necessarily share defining attributes with each other but only with the central subcategory—hence the term radial, which refers to this internal structure.

One of Lakoff's ordinary-language examples of a radial category is "mother" (1987, 83–84). Here the central subcategory corresponds to an individual who, in the context of conventional gender relations in the United States, is often considered a "true" mother—that is, one who (1) is a woman, (2) contributes half the child's genetic makeup, (3) bears the child, (4) is the wife of the father, and (5) nurtures the child. The noncentral subcategories arise when the component elements are taken singly, or in sets of two or more. In this example, familiar types emerge if these roles are taken singly: "genetic mother," "birth mother," "stepmother," and "nurturing mother."

Radial categories merit attention here because they play an important role in the language of social science. For example, following Ostiguy (1993), one can view "democracy" as a radial category. Obviously, the problem of identifying the components of democracy has long been a matter of debate. For present illustrative purposes, the following partial definition will suffice.[12] We might say that the central subcategory "democracy" is constituted by elements such as (1) broad and effective participation in the process of rule, (2) limitation of state power and protection of individual rights, and (3) according to some accounts, egalitarian (or at least relatively more egalitarian) economic and social relationships. The first component taken alone might be seen as constituting the noncentral subcategory "participatory democracy," the first and second combined as constituting "liberal democracy," and the first and third combined as constituting what may be called "popular democracy."

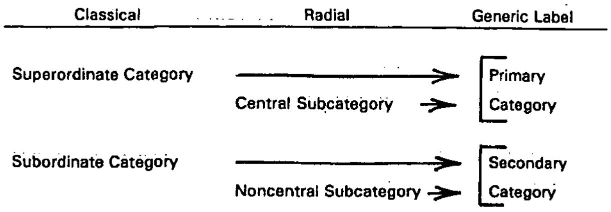
Comparing Radial and Classical Categories

The internal form of radial categories differs from that of classical categories. The variants that branch out within a radial structure such as "mother" or "democracy" might be viewed as subsets of the overall category. Yet they do not share the full complement of attributes by which we would recognize the overall category, as they do with classical categories. Rather, they *divide* them. This difference has important implications for how these two types of categories are used in comparative analysis.

Before radial and classical categories are compared further, an issue of labeling should be clarified. We have referred to the component elements of classical categories as superordinate and subordinate, whereas for radial categories we have referred to central and noncentral subcategories. For the sake of comparison, we can apply more generic labels (see Figure 3). The term *primary category* will be used to refer to the overall category, whereas *secondary category* will be used to refer to the category whose

FIGURE 3

Generic Labels for Comparing Classical and Radial Categories



meaning is derived from the primary category. Thus, "mother" and "democracy" are primary categories, and "birth mother" and "liberal democracy" are the corresponding secondary categories. In the realm of classical categorization, "authoritarianism" is a primary category, and "bureaucratic authoritarianism" is the corresponding secondary category.

The contrast between classical and radial categories may now be examined, first using Lakoff's examples of "dog," a classical category in the scheme of traditional taxonomy, and "mother," a radial category (1987, 46, 74–76). As can be seen in Figure 4, in the case of the classical category of "dog" the differentiating attributes of the secondary categories occur *in addition to* those of the primary category. By contrast, with the radial category of "mother" the differentiating attributes of the secondary categories are *contained within* the primary category.

The examples of "authoritarianism," understood as a classical category,[13] and "democracy," understood as a radial category, yield the same contrast (Figure 5). In relation to authoritarianism, the differentiating attributes of the secondary categories of "populist" and "bureaucratic" authoritarianism occur *in addition to* those of the primary category. In the case of "democracy," the differentiating attributes associated with "participatory," "liberal," and "popular" democracy are *contained within* the primary category.

This contrast between the two types of categories

FIGURE 4

Differentiation of Classical and Radial Categories

Classical Category: Dog

	Category	Components					
Primary Category	Dog	A	B	C			
Secondary Categories	Retriever	A	B	C	D		
	Sheepdog	A	B	C		E	
	Spaniel	A	B	C			F

Note: Differentiating characteristics of secondary categories are *in addition to* those of the primary category.

A, B, and C = Hypothetical set of general attributes of dogs
D, E, and F = Hypothetical attributes that differentiate specific types of dogs

Radial Category: Mother

	Category	Components				
Primary Category	Mother	A	B	C	D	E
Secondary Categories	Genetic mother	A	B			
	Birth mother	A		C		
	Nurturing mother	A			D	
	Stepmother	A				E

Note: Differentiating characteristics of secondary categories are *contained within* the primary category.

A = Female
B = Provides 50% of genetic makeup
C = Gives birth to child
D = Provides nurturance
E = Married to father

FIGURE 5

Differentiation of Classical and Radial Categories: Examples from Regime Analysis

Classical Category: Authoritarianism

	Category	Components			
Primary Category	Authoritarianism	A	B		
Secondary Categories	Populist Authoritarianism	A	B	C	
	Bureaucratic Authoritarianism	A	B		D

Note: Differentiating characteristics of secondary categories are *in addition to* those of the primary category.

A = Limited pluralism*
B = Distinctive mentalities, not guiding ideology*
C = Substantial mobilization of working class and/or middle class
D = Alliance of military, technocrats, and transnational capital against previously mobilized popular classes

Radial Category: Democracy

	Category	Components		
Primary Category	Democracy	A	B	C
Secondary Categories	Participatory democracy	A		
	Liberal democracy	A	B	
	Popular democracy	A		C

Note: Differentiating characteristics of secondary categories are *contained within* the primary category.

A = Effective political participation
B = Limitation of state power
C = Social and economic outcomes of relative equity

* The definition of authoritarianism employs two elements used by Linz (1975).

has a major practical consequence in terms of how we go about addressing the problem of conceptual stretching: the extension of the secondary category in radial categorization may exceed that of the primary category. Consider an example from common usage: a woman who is a birth mother might not be seen as fitting the overall category of what is understood as a "true" mother.[14] All mothers that fit the primary category (i.e., all "true" mothers) are birth mothers, but the converse is not the case. Hence, there are more birth mothers in the world than "true" mothers.

The same pattern appears with democracy. If only the extensive political participation associated with democracy is present in a given country, without protection of the rights of those who at any given time may be in a minority, many observers will conclude that it is not what they consider to be a "true" democracy. Here again, the extension of the secondary category will exceed that of the primary category, involving the same inverse relationship between extension and intension already discussed.

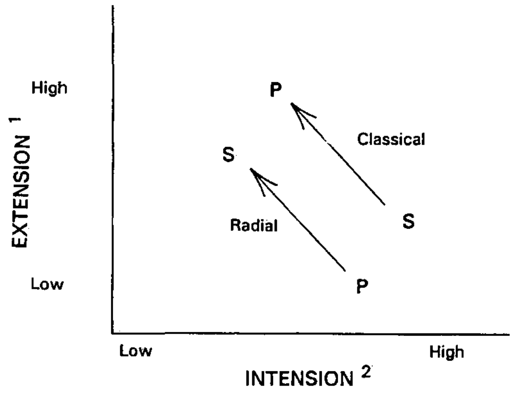
Authoritarianism Versus Democracy: Contrasting Patterns of Category Change

Let us apply these ideas to two examples of conceptual traveling. During an earlier period of wide interest in bureaucratic authoritarianism, that category was at times extended to cases that only marginally fit the original meaning (Collier 1979, 1993). Using the ladder of generality, scholars sometimes avoided this problem of conceptual stretching by shifting to the broader category of authoritarianism.

A parallel problem has arisen with recent efforts to apply the category "democracy" to new regimes in Central America, Eastern Europe, and the former Soviet Union. In some of these cases, where leaders are selected in competitive elections but where many of the institutions and practices often associated with democracy are absent, the problem of conceptual stretching may be addressed by making the more modest claim that these are, for example, "electoral democracies," thereby abandoning the implication that they are "true" democracies.

FIGURE 6

Avoiding Conceptual Stretching with Classical and Radial Categories



P = Primary category
S = Secondary category

1 Range of cases
2 Number of defining attributes

The contrast that emerges here must be kept in mind as scholars seek to avoid conceptual stretching in comparative research. In the case of bureaucratic authoritarianism, this undesirable outcome is avoided by moving up a ladder of generality from a secondary category to the primary category. In the case of democracy, it is avoided by moving *away* from the primary category to employ a secondary category.

Figure 6 summarizes this contrast between classical and radial categories in the framework of a diagram of extension and intension like Figure 1. As can be seen in Figure 6, with classical categories, to avoid conceptual stretching one moves from the secondary category, S, to the primary category, P, by rising up a line of negative slope like that of Figure 1. With radial categories, by contrast, to avoid conceptual stretching one moves up a parallel line, but from the primary category, P, to the secondary category, S.

Two further contrasts between radial and classical categories may be noted. First, an important difference is evident in how the formal label is modified as one moves from one level of generality to another. With both types of categories, it often (though not always) occurs that primary categories are made into secondary categories by adding an adjective. Thus, "bureaucratic authoritarianism" is a secondary category in relation to "authoritarianism," and "electoral democracy" is a secondary category in relation to "democracy." This similarity helps to underline a crucial contrast in how we move to a broader set of cases with classical, as opposed to radial, categories. In the example of the classical category "bureaucratic authoritarianism," this is done by *dropping* an adjective. By contrast, with the radial category "democracy," it is done by *adding* an adjective. Thus, the analyst seeking to avoid conceptual stretching will use adjectives in opposite ways, depending on the type of category in question.

Second, in the case of radial categories, the possibility of encompassing more cases through the elaboration of secondary categories can allow for considerable flexibility regarding the meaning and application of the category. Although this flexibility is often desirable, it can be the source of major scholarly debates. For example, as scholars seek to identify new subtypes of democracy, disputes can easily arise as to whether it is appropriate to consider the cases that fit these subtypes to be "truly" democratic.[15] By contrast, in analyses of a classical category such as "bureaucratic authoritarianism," no parallel debate emerged about whether the cases of bureaucratic authoritarianism were instances of "true" authoritarianism.[16]

Further Illustrations of Radial Structure in Recent Discussions of Democracy

Recent analyses of democracy by Terry Karl and Philippe Schmitter illustrate some of the concomitants of the radial structure of this category (Karl 1990, 2; Schmitter and Karl 1991, 76–82; idem 1992, 52). Of the three attributes of democracy we have discussed, Karl and Schmitter deliberately set aside the questions of equity raised above and focus on issues associated with participatory democracy and liberal democracy. Summarizing schematically, we may say that they are concerned with four elements: "(1) *contestation* over policy and political competition for office; (2) *participation* of the citizenry through partisan, associational, and other forms of collective action; (3) *accountability* of rulers to the ruled through mechanisms of representation and the rule of law" (Karl 1990, 2; emphasis added); and (4) *protection of rights* essential to meaningful contestation, participation, and accountability.[17]

Karl explicitly notes what we see as an essential component of the radial structure of this category. In a discussion of subtypes of democracy (which we call the secondary categories), she observes that they "are characterized by different mixes and varying degrees of the chief dimensions of democracy: contestation, participation, [and] accountability" (1990, 2). Thus, she recognizes the essential point evident in Figures 4 and 5: secondary categories tend to divide up the component elements of the primary category, and they may vary considerably in how closely they resemble the central subcategory.

This pattern also appears in the subtypes developed jointly by Schmitter and Karl (1992, 56–58). They identify "corporatist" democracy and "populist" democracy in part by the shared attribute that the dominant center of power is located in the state. Clearly, this attribute mitigates the weight of other components of their understanding of democracy, such as citizen participation and the accountability of rulers. Thus, in their framework these subtypes are less democratic than what might be deemed to be "true" democracies. The fact that these subtypes are seen as less democratic comes out clearly in Schmitter and Karl's empirical analysis of 24 recent cases of democratization. Of the eight countries they assign to the subtypes of "populist" and "electoralist" democracy, they treat six as marginal cases, either because they have "not yet crossed the minimal democratic threshold" or because they "are not yet consolidated into a recognizable type of democracy" (p. 68).

Schmitter and Karl's analysis brings us back to our argument about conceptual traveling. It would appear that one of their goals is to introduce a broad range of empirical cases into the debate on democratization, yet without stretching the concept. Along the lines of our discussion, they attempt to do this by creating secondary categories (e.g., "corporatist" and "populist" democracy) referring to cases that observers might hesitate to call "true" democracies. These secondary categories serve to increase the extension of the overall category, without distorting it. In this way, the authors bring these cases into the framework of a general discussion of democracy, without having to claim that they are all truly democratic.

This elaboration of the category by Schmitter and Karl may be placed in perspective through comparison with an innovation proposed some time ago by Robert Dahl (1956, 1963, 1971). Dahl argues that for the analysis of concrete cases, it is more productive to employ the term *polyarchy*, rather than *democracy*. He uses democracy to refer to "an unattained and perhaps unattainable ideal," whereas polyarchy refers to existing political systems that could be seen as "relatively (but incompletely) democratized" (Dahl 1963, 73; idem 1973; 8). To avoid conceptual stretching, Dahl uses distinct labels for the idealized version of the category and for the version that refers to actual cases. Schmitter and Karl's treatment differs from Dahl's in two ways: (1) in their usage, the term democracy refers to at least some cases, rather than to a hypothetical ideal, and (2) instead of using separate label to extend the category to more cases, they avoid conceptual stretching by adding adjectives to the existing label. However, the treatments are similar in that Dahl, like Schmitter and Karl, creates a secondary category (i.e., a noncentral subcategory), following a radial pattern. Dahl's term *polyarchy* might be thought of as a "catch-all" secondary category in relation to the primary category democracy; that is to say, using polyarchy to refer to relatively democratized systems is the functional equivalent of adding an adjective to create the secondary category "partial" democracy or "incomplete" democracy in order to capture a larger number of partial cases.

To summarize, the radial category "democracy" has a structure that, through the elaboration of secondary categories, allows for wide variation in meaning and application within a generally agreed-upon area of discussion. Yet whether these variations in meaning and application are accepted or contested within the scholarly community is an abiding issue.

CONCLUSION

The goal of this discussion has been to offer new guidelines for comparative analysts who are concerned with the problems of conceptual traveling and conceptual stretching. We conclude that Sartori's framework for addressing these problems deservedly remains a benchmark for scholars of comparative politics. Yet some caution and refinement are in order.

The examination of family resemblances reminds us that an overly strict application of classical principles of categorization can lead to the premature abandonment of potentially useful categories. This problem can be avoided by self-consciously thinking in terms of ideal types, by using a system-specific approach to applying categories in particular contexts, or by adopting other techniques that do not depend on the assumption that members of a category share a full set of defining attributes.

The effort to avoid conceptual stretching must likewise take a somewhat distinct form when one is dealing with radial categories. This is the case because, with such categories, what we have called the secondary category (e.g., "electoral democracy") tends to *divide up* the constituent elements of the primary category ("democracy"). By contrast, with classical categories, the secondary category (e.g., "bureaucratic authoritarianism") tends to contain *additional* elements beyond those of the primary category ("authoritarianism"). As a consequence, with radial categories, the secondary category may have greater extension, whereas with classical categories, the primary category has greater extension. Relatedly, with classical categories one may often avoid conceptual stretching by removing an adjective, whereas with radial categories one may often avoid conceptual stretching by adding an adjective.

We also argue that because the secondary categories tend to divide up elements of a radial category such as "democracy," the formation of secondary categories creates both an opportunity and a problem. It creates an opportunity for broader and more flexible application by increasing the category's extension. Yet this very flexibility can lead to major scholarly disputes about whether the category fits the cases under study.

A final observation may be made about this central issue of the fit between categories and cases. Insights into the structure of categories do not tell us everything we need to know about how to apply them in research. Rather, this application depends on substantive expertise regarding the cases under analysis. We have suggested the example of a debate on whether a particular case should be called an instance of patrimonial, as opposed to traditional, authority. Though our methodological understanding of categories can frame such a debate, its resolution requires knowledge of the cases. In this sense, the arguments about categories that have been our focus here play the useful role of bringing us back to our own detailed understanding of the political settings we study.

Looking beyond these guidelines, we recognize that various issues raised here require further examination. More analysis is needed of the relation between classical and radial categories. Whereas some categories unambiguously correspond to one of these types, others may contain elements of both. Further, in pursuit of particular analytic goals, social scientists deliberately modify categories, often attempting to impose a classical structure on radial categories.[18]

These attempts to modify categories raise the larger issue of the relation between ordinary and technical language. When scholars create a technical language, they may well succeed in achieving greater clarity and consistency or in highlighting what they view as important aspects of the phenomena they study. On the other hand, it is possible that this new language will not be anchored in the familiar linguistic prototypes that play such an important role in making categories interesting and vivid. The modified categories might fail to gain currency, perhaps being displaced by more familiar usage.

This tension between the advantages and pitfalls of modifying categories raises the question of the proper task of methodology. To what extent should an understanding of how we tend to use categories inform our judgment about how we ought to use them? Should the methodological analysis of categories emphasize *description*, which might encourage realism about the constraints imposed by ordinary language on technical usage, or *prescription*, which might recommend means to overcome these constraints?[19] We have tried here to give reasons for attending to both.

Notes

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1. We treat concepts and categories as similar. Here we use the term *category* because it seems to point more directly to the issue of boundaries (a central concern of this analysis) and because it follows the usage of Lakoff (1987), whose work we build upon. Sartori, whom we cite extensively, refers to "concepts," instead of "categories." Our usage is not intended in a spirit of disagreement with his: it is simply more helpful for present purposes. But we retain his usage in such special expressions of his as *conceptual traveling* and *conceptual stretching*.

2. The founders of this school include Bendix (1956, 1964), Lipset and Rokkan (1967), and Moore (1966). Subsequent work includes Bendix 1978; Bergquist 1986; Collier and Collier 1991; Goldstone 1991; Luebbert 1991; Paige 1975; Ragin 1987; Rokkan 1970; Rueschemeyer, Stephens, and Stephens 1992; Skocpol 1979, 1984; Tilly 1975, 1984; and Trimberger 1978.

3. For authoritarianism and corporatism, see Anderson 1970; Berger 1981; Collier 1979; Lembruch and Schmitter 1982; Linz 1975; Linz and Stepan 1978; Malloy 1977; O'Donnell 1973; Schmitter 1971, 1974; Schmitter and Lembruch 1979; Stepan 1973, 1978. For democratization, see Di Palma 1990; Huntington 1991; O'Donnell, Schmitter, and Whitehead 1986; Pastor 1989; and the new *Journal of Democracy*.

4. The task of developing an adequate typology of different forms of categories and of situating these three types (classical, family resemblance, and radial) within it goes well beyond our goal here, which is simply to explore certain contrasts among these three types in order to illustrate some dilemmas that arise as researchers extend their categories to more cases. For a broad overview of different forms of categorization, see Lakoff 1987. For a brief discussion of the relation of ideal types to family resemblance and radial categories, see n. 8.

5. Sartori refers to a ladder of "abstraction" (1970, 1040; 1984, 44–46). However, because the term *abstract* is often understood in contrast to *concrete*, this label can be confusing. We therefore find that it expresses our meaning more clearly to refer to a ladder of generality.

6. We are aware that a more complete formulation of the law of inverse variation entails the expectation that an increase in the intension implies either decreasing, or simply nonincreasing, extension (Angeles 1981, 141). In this formulation, the slope would be either negative or zero (i.e., horizontal). However, we follow Sartori's usage, which for present purposes adequately captures the pattern of variation.

7. For the purpose of the present discussion, we do not consider the additional characteristic that Wittgenstein attributes to this type of category, namely, that cases may have differing degrees of centrality within the category.

8. The commonalities between family resemblance (and also radial categories) and Weberian ideal types are made very clear in Burger's insightful interpretation of Weber's theory of concept formation (1976, 115–16, 156–57). Burger emphasizes that Weber's use of ideal types grew out of his recognition that the most interesting concepts are not based on defining properties shared by all cases to which the concept refers (and thus do not follow the pattern of classical categorization). Weber, therefore, embraced the use of ideal types in which key attributes associated with the concept were expected to be present to varying degrees. Procedures we discuss for making generalizations with family resemblances and radial categories are thus of considerable relevance to ideal types as well.

9. Their presentation is couched in the language of measurement; and they refer to indicators, rather than attributes. However, they have a broad understanding of measurement as entailing an "ordered language" that serves "for the expression of empirical observations" (Przeworski and Teune 1970, 11), which is essentially what is understood here as concept formation.

10. For example, forming the concept is essential when one seeks to differentiate, within a set of highly intercorrelated attributes, between those attributes which are components of the concept and those which are *causes* or *consequences* of the concept.

11. See note 8 concerning the relationship between radial categories and ideal types.

12. We do not attempt to take account here of the vast literature that has analyzed democracy and its components and dimensions.

13. A fuller analysis of authoritarianism could reveal certain respects in which it departs from the classical model. However, its usage appears in major respects to have a classical form.

14. We recognize that this usage is contested (see Gallie 1956). With changes in gender relations and in the legal framework within which they are situated, the usage may change. The point here is that regardless of the outcome of

such disputes, the secondary category will often have greater extension than the primary category.

15. For a discussion of democracy as a "contested concept," see Gallie 1956.

16. In his assessment of post-1964 Brazil, Linz drew attention to the poorly institutionalized character of the prevailing political institutions by referring to them as constituting an "authoritarian situation" rather than "authoritarian regime" (1973, 235). However, their authoritarian character was not at issue.

17. We find it helpful to view Karl's fourth element, civilian control over the military, as an aspect of accountability. We have added a different fourth element, the protection of rights, to capture related issues discussed by Schmitter and Karl (1991, 1992).

18. Recent efforts to develop "minimal" or "minimalist" definitions of democracy which build on the earlier work of Schumpeter (1950), represent an effort to shift the category of democracy toward a classical pattern (Di Palma 1990, 28; Huntington 1991, 9). On the other hand, Ostiguy (1993) is attempting to push scholars toward recognizing its radial structure. With reference to the category of authoritarianism, we have treated it as classical, thereby following what seems to be the generally accepted usage. Yet Linz's (1964) pioneering article bases the discussion on the analysis of a prototype, Franco's Spain. To the extent that scholarly understanding of the category is strongly influenced by such a prototype, an element more characteristic of radial categories is thereby introduced.

19. Among philosophers, research in cognitive science has sparked a parallel debate about the proper role of epistemology. See Bealer 1987; Churchland 1987; and Goldman 1987.

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INCENTIVES TO CULTIVATE FAVORED MINORITIES UNDER ALTERNATIVE ELECTORAL SYSTEMS

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A simple model is used to compare, under different electoral systems, the incentives for candidates to create inequalities among otherwise homogeneous voters, by making campaign promises that favor small groups, rather than appealing equally to all voters. In this game model, each candidate generates offers for voters independently out of a distribution that is chosen by the candidate, subject only to the constraints that offers must be nonnegative and have mean 1. Symmetric equilibria with sincere voting are analyzed for two-candidate elections and for multicandidate elections under rank-scoring rules, approval voting, and single transferable vote. Voting rules that can guarantee representation for minorities in multiseat elections generate, in this model, the most severely unequal campaign promises.

In an election campaign with many competing candidates, a candidate can try to appeal broadly to all voters equally or concentrate more narrowly on winning the support of minorities or special interest groups. Which strategy is better for winning an election? The answer to this question may depend crucially on the rules of the electoral system.

My focus is on the creation of favored minority groups in election campaigns, so I consider situations in which all voters are initially the same and equilibria that are symmetric both across candidates and across voters. Nonetheless, some electoral systems may encourage candidates to build winning support by proposing transfer schemes for the benefit of a small minority of voters, thus breaking the symmetry among voters and creating favored minorities by their campaign promises. Under plurality voting, for example, if there are 10 candidates in the race, then a candidate could easily win with only 20% of the vote. Thus a campaign promise to tax 80% of the voters for the benefit of a 20% minority could be a successful strategy. We will show, however, that other electoral systems (such as Borda voting and approval voting) may significantly reduce such incentives to favor minority interests.

In general, there are several different reasons why a politician may want to make campaign promises that pander to special interest groups. A candidate may make promises to a special group of voters in order to get votes from them, to get donations for campaign expenses, or to get bribes and kickbacks for personal use. I shall focus exclusively on the first of these incentives. (The second requires a theory of how campaign spending influences votes, for which see, e.g., Morton and Myerson 1992. For the third, see Myerson 1993.)

A candidate who could get away with it might try to win by promising everything to every voter. So we must assume that there are some limits on a candidate's ability to misrepresent him- or herself as all things to all people. I shall assume full rational expectations, so that each candidate must make promises that he or she can actually fulfill if elected. Thus, in our model, each candidate's campaign promises must satisfy a budget constraint, which is determined by the quantity of public resources that the candidate would be able to allocate in case of winning the election.

For example, suppose that there are one million voters and that a winner of the election would have one million dollars of public resources to allocate among these voters. Then a candidate cannot make promises that average more than $1.00 per voter. The equitable solution would be, of course, to promise $1.00 to every voter. In a two-candidate race, such an equitable candidate would lose, however, if the second candidate promised $1.25 to 80% of the voters and nothing to the other 20% of the voters, which is also credible, because the promises average to $1.00 per voter. The first candidate could beat the second candidate, by switching to a strategy of promising $1.50 to half of the voters and $.50 to the other half, but this strategy could also be beaten by other feasible strategies for the second candidate.

To identify optimal strategies in a Nash equilibrium, we must first specify the technical details of the game and keep the formulation simple enough to be tractable. I shall here consider one such simple formulation. To introduce the model in its simplest case, I begin by considering the two-candidate case. The analysis is then extended to cover all rank-scoring rules with any given number of candidates. The characterization of the unique symmetric equilibrium for general rank-scoring rules is my main result (Theorem 2). I shall then analyze the equilibrium offer distributions for approval voting and for single transferable vote and extend these results to elections for multiseat councils.

Finally, I shall examine the close relationship between my results and Cox's (1987, 1990) analysis of centripetal and centrifugal incentives in electoral systems. I find that electoral systems that, according to Cox's analysis, encourage more diversity of candidates' positions generally also incite candidates to create more inequality among voters. That is, electoral systems that encourage candidates to advocate the interests of existing minorities may also incite candidates to use narrow campaign strategies that *create* favored minorities even in situations where all voters are initially the same. These results should offer insights of practical importance for the constitutional design of democratic institutions.

TWO-CANDIDATE ELECTIONS

We begin by studying a race between two candidates, numbered 1 and 2, who are competing for some office. Each voter votes for one of the two candidates, and the winner is the candidate with the majority of the votes. (In a winner-take-all two-candidate election, this is essentially the only neutral anonymous electoral system, so comparison of electoral systems will have to be deferred until we consider multicandidate elections.)

In the model, each candidate's strategy for generating campaign promises is described mathematically by an *offer distribution,* which is a probability distribution over the nonnegative real numbers. In this offer distribution, the probability of any interval may be interpreted as the fraction of the voters for whom the candidate's offer of government benefits has a value that is in this interval. Equivalently, if a candidate's offer distribution has a cumulative probability function $F(\cdot)$, then $F(x)$ denotes the probability that a randomly sampled voter will be someone to whom the candidate has promised benefits that are worth less than x dollars.

Given that we want to investigate questions about the relative equality or inequality of the distribution of public benefits, the simplest possible way to formulate the government's service technology is to assume that benefits are derived purely from the allocation of linearly divisible resources. Thus, each candidate's budget constraint is expressed, in the model, as a constraint on the average offer per voter that a candidate can promise. Specifically, I assume that each candidate's offer distribution must have mean 1, to be considered credible by the voters.

To avoid specifying the number of voters and dealing with the complexities of large finite numbers, I shall consider the number of voters to be essentially infinite. We should, however, interpret such an infinite model as an approximation to a large finite population with millions of voters. Under this interpretation, the budget constraint tells us that the winner of the election will control resources that have a monetary value that is equal in magnitude to the number of voters. The winner will divide these resources and allocate them to the voters consistently with campaign promises.

If candidate 2 could make offers after observing candidate 1's offers to all voters, then candidate 2 could always win the election. For example, candidate 2 could identify a small group of voters who are promised the most by candidate 1 (e.g., the top 10% in candidate 1's distribution) and offer nothing to this group. Then candidate 2 could offer every other voter slightly more than candidate 1 promised, financing the excess over candidate 1's offers from the resources not given to the voters in the first group. Every voter outside of the first small (10%) group would vote for candidate 2, who would win by a landslide (90%). To avoid this simple outcome, I assume that the two candidates must make their campaign promises independently.

I want to consider an electorate that is initially homogeneous, before the candidates make campaign promises. Thus, I assume that candidates' offers are independent across individual voters, so that no voter's offers have any specific relationship with any other set of voters' offers. That is, each voter's offer from each candidate is assumed to be drawn from the candidate's offer distribution independently of the candidate's offers to all other voters, and independently of all other candidates' offers to all voters. This independence assumption greatly simplifies the analysis, because it allows me to completely characterize a candidate's campaign promises by the marginal distribution of the candidate's individual offers to voters, without saying anything more about the joint distribution of offers to various sets of voters. The infinite-population assumption was introduced essentially only to justify this simplifying assumption of independence across voters.[1]

We also assume that all offers must be nonnegative. One way to justify a nonnegativity assumption is to suppose that taxes are not in question in this campaign and that the candidates are only debating how the tax revenues should be redistributed to the voters. A more sophisticated model would allow taxes to be variable but would recognize that tax-avoidance activities would increase the total economic cost of raising each additional dollar of revenue as the tax rate increases. In such a framework, the model can be justified by supposing that there are no such tax-avoidance costs as long as taxes are below some maximal level but that tax-avoidance activities would make it infinitely costly to extract any additional tax revenue above this maximal level. To justify the 1-mean budget constraint, we can redefine the unit of money (if necessary) so that these maximal taxes would average one monetary unit per voter.

Thus, at the beginning of the game, each candidate i simultaneously and independently chooses an offer distribution. We may represent candidate i's offer distribution by a cumulative distribution function F_i, where $F_i(x)$ denotes the fraction of the voters to whom candidate i will offer less than x. Each offer distribution must have mean 1, and so F_i must be a nondecreasing function that satisfies $\int_0^\infty x \, dF_i(x) = 1$, as well as $F_i(x) = 0, \forall x \leq 0$ and $\lim_{x \to \infty} F_i(x) = 1$. Next, each voter goes to each candidate's campaign headquarters and gets an offer that is randomly drawn from the candidate's offer distribution, independently of all other offers. (We may suppose that the voter must show a voter's identification number to get a draw, in order to fix it verifiably and prevent the voter from coming back for a second draw if it is low.) Then, on election day, each voter knows how much to expect from each candidate and votes for the candidate who has promised more. The winning candidate is the one who gets the most votes.

Let p_{ih} denote the probability that a randomly sampled voter would prefer candidate i over candidate h. Thus in Riemann–Stieltjes integral notation,[2] $p_{ih} = \int_0^\infty F_h(x)\, dF_i(x)$. That is, the probability that any given voter prefers i to h is the expected value of $F_h(x)$ (the probability that h offers less than x to this voter) when x a random variable drawn from the F_i offer distribution. With a voting population approaching infinity, the fraction of the voters who prefer i over h is almost surely p_{ih}. Thus candidate 1 wins (almost surely) if $p_{12} > p_{21}$, and candidate 2 wins if $p_{21} > p_{12}$. I assume throughout that each candidate is motivated purely by the objective of winning the election. Thus, we can formulate the candidates' competition as a two-person zero-sum game in which the payoffs to candidates 1 and 2 are $(1, -1)$ if $p_{12} > p_{21}$, $(-1, 1)$ if $p_{12} < p_{21}$, and $(0, 0)$ if p_{12} and p_{21} are equal.[3]

Because this game is symmetric, both candidates must get expected payoffs of 0 in equilibrium. I look for a symmetric equilibrium, in which both candidates use the same offer distribution.

THEOREM 1. *In a two-candidate race, the unique symmetric equilibrium is for each candidate to generate offers from a uniform distribution over the interval from 0 to 2; that is, the cumulative distribution* F *of the equilibrium offer distribution for each candidate must satisfy* $F(x) = x/2$ *if* $0 \le x \le 2$.

This result is a special case of Theorem 2, which is proven in the next section, so I do not give a formal proof of uniqueness here. However, it is worthwhile to verify here that this uniform distribution on [0, 2] does give us an equilibrium. When both candidates draw their offers independently from the uniform distribution, they each expect to get half of the votes, of course. If candidate 1 stays with the uniform distribution on [0, 2] while candidate 2 deviates to some other distribution F_2, then candidate 2's expected vote share is

$$\int_0^\infty F_1(x)\, dF_2(x) \le \int_0^\infty (x/2)\, dF_2(x)$$

$$= \left(\int_0^\infty x\, dF_2(x)\right)/2 = 1/2$$

(because $F_1(x) = x/2$ when $0 \le x \le 2$, $F_1(x) = 1 \le x/2$ when $x > 2$, and the mean of candidate 2's distribution must equal 1). So candidate 2's vote share cannot be increased by deviating to another distribution.

This result is not really new. Game theorists since Gross and Wagner (1950) have found such uniform distributions in equilibria of "Colonel Blotto" games, where each of two competitors divides a fixed supply of resources over a set of battlefields (see also Owen 1982, 78–83; Shubik 1970). To apply the "Colonel Blotto" terminology to our context, we can reinterpret battlefields as voters, for whose votes the candidates compete with their offers. The hardest part of Gross and Wagner's problem was to construct joint distributions for allocations that always sum to the given total but give uniform marginal distributions for each battlefield/voter. I have avoided such difficulties here by allowing the offers to be made independently to the various voters and by only requiring that the budget constraint be satisfied in expected value.

The advantage of my simplified formulation is that it will enable us to go beyond this "Colonel Blotto" literature and get results about more complicated situations in which more than two candidates are competing. Such games will allow us to get important new insights into the comparison of multicandidate electoral systems.

MULTICANDIDATE COMPETITION WITH RANK-SCORING RULES

I now consider an election in which there are K candidates for a single indivisible office and the voters vote according to a *rank-scoring* election rule. Such an electoral system can be characterized by an ordered sequence of K numbers, which I denote here by $s_1, s_2, \ldots, s_K$, where $1 = s_1 \ge s_2 \ge \ldots \ge s_K = 0$. Without any loss of generality, I normalize the highest and lowest point values to be 1 and 0, respectively. In the election, each voter must indicate a ranking of the K candidates on a ballot; then this voter's ballot gives the top-ranked candidate s_1 points, the second-ranked candidate s_2 points, and so on, with the jth ranked candidate getting s_j points. Each candidate's *score* is the average of the points received from all voters. The winner of the election is the candidate with the highest score.

Plurality voting, or *single nontransferable vote* (SNTV) is a rank-scoring rule in which $s_1 = 1$ and $s_j = 0$ for all $j > 1$. More generally, for any V between 1 and K, a rule where each voter must distribute V *noncumulative votes* on the ballot is represented by letting $s_1 = s_2 = \ldots = s_V = 1$, and $s_j = 0$ for all $j > V$. *Negative-plurality voting* (in which each voter votes against one candidate and the winner is the candidate with the fewest such votes) can be characterized as a rank-scoring rule in which $s_j = 1$ for all $j < K$ and only $s_K = 0$. *Borda rule* is a rank-scoring rule in which $s_j = (K - j)/(K - 1)$, for each j.

Given any rank-scoring rule $(s_1, s_2, \ldots, s_K)$, let $\bar{s}$ denote the average of the ranking points that a voter can give, so

$$\bar{s} = \sum_{j=1}^{K} s_j/K.$$

Thus, $\bar{s} = V/K$ in a voting system where each voter must vote for exactly V of the K candidates, and $\bar{s} = 1/2$ for Borda rule.

I assume, as before, that at the beginning of the campaign each candidate must choose an offer distribution, which is a probability distribution over the nonnegative real numbers that has expected value equal to 1. During the campaign, each voter gets an offer or promised payoff from each candidate that is independently drawn from the candidate's offer distribution. Thus, a distribution in which most of the probability mass is near 1 would denote a campaign strategy of appealing broadly to most of the voters. On the other hand, a campaign strategy of offering special favors to small groups would be denoted by a distribution that has positive mass at numbers much higher than 1, balanced by a large mass near 0.

A symmetric equilibrium of this game is a scenario in which every candidate is expected to use the same offer distribution, and each candidate finds that using this offer distribution maximizes the chances of winning the election, when he or she knows that all other candidates are also independently allocating their offers according to this distribution, and all voters perceive that the K candidates all have the same probability of winning the election. I here focus exclusively on finding such symmetric equilibria. (For the existence and significance of nonsymmetric equilibria of voting games, see Myerson and Weber 1993 and Myerson 1993.)

As a consequence of the symmetry assumption, we can here suppose that the voters vote sincerely. Each voter thinks that in the event that his or her vote could actually decide a close race between two of the candidates, it is equally likely to be any pair of the K candidates who are in this close race. (In Myerson and Weber's [1993] terms, I am assuming that each voter perceives that all of the $K(K-1)/2$ pivot probabilities are equal.) Thus, each voter should rank the candidates in order of their offers, giving s_1 points to the candidate who offers the most, s_2 points to the candidate who offers the second-most, and so on.

Consider the situation faced by a given candidate i who chooses on offer distribution, assuming that each other candidate will use the equilibrium offer distribution. Let $F(x)$ denote the cumulative probability that any given voter will be offered less than x by any other given candidate, according to this equilibrium distribution. When candidate i offers x to a voter, the probability that this candidate i will be ranked in position j by this voter is $P(j,F(x))$, where we let

$$P(j,q) = q^{K-j}(1-q)^{j-1}(K-1)!/((K-j)!(j-1)!),$$

where ! denotes the factorial operator, so that $(K-1)! = 1 \times 2 \times \ldots \times (K-1)$. That is, $P(j,q)$ denotes the probability that exactly $K-j$ of the $K-1$ other candidates will offer the voter less than x, given that each other candidate has an independent probability q of offering less than x to this voter. Thus, if candidate i offers x to a voter, then the expected value of the points that this voter will give to this candidate is $R(F(x))$, where we let

$$R(q) = \sum_{j=1}^{K} P(j,q)s_j.$$

(Things could be slightly more complicated if there were a positive probability of other candidates offering exactly x, but we can ignore such complications here. I shall show in the proof of Theorem 2 that the equilibrium distribution cannot assign positive probability to any single point.)

When all candidates independently use the same offer distribution, they must all get the same expected score, which must equal $\bar{s}$, the average point score given by each voter. Thus there is a symmetric *equilibrium* in which all candidates use the cumulative distribution F if and only if, for any other distribution G that is on the nonnegative numbers and has mean 1, $\int_0^\infty R(F(x))\,dG(x) \leq \bar{s}$.

THEOREM 2. *In a K-candidate election under the rank-scoring rule with ranking points* $(s_1, s_2, \ldots, s_K)$, *there is a unique symmetric equilibrium of the candidates' offer-distribution game. In this equilibrium, each candidate chooses to generate offers according to a distribution that has support on the interval from 0 to* $1/\bar{s}$ *and that has a cumulative distribution* $F(\cdot)$ *that satisfies the equation*

$$x = R(F(x))/\bar{s}, \quad \forall x \in [0, 1/\bar{s}].$$

Proof. To find an equilibrium, we show first that the equilibrium offer distribution must be continuous, that is, it cannot have any points of positive probability. If all candidates used an offer distribution that assigned a positive probability δ to some point x, then there would be a positive fraction (δ^K) of the voters who would be exactly indifferent among the candidates. Any candidate could then move his or her average point score among this block from $\bar{s}$ up to s_1 by giving an arbitrarily small increase (say, ε) to most of the voters to whom the candidate was going to offer x; and the cost of this increase $(\varepsilon\delta)$ could be financed by moving an arbitrarily small fraction (ε/x) of this block down to zero. That is, if the distribution had a positive mass at some point, then a candidate could gain a positive block of votes by a transfer of resources that would lower the score from only an arbitrarily small number of voters.

Applying the definitions of $P(\cdot,\cdot)$ and $R(\cdot)$, we now show that

$$R(0) = s_K = 0, \; R(1) = s_1 = 1;$$

and $R(\cdot)$ is a continuous and strictly increasing function over the interval from 0 to 1. These equations hold because $P(j, 0) = 0$ unless $j = K$; $P(j, 1) = 0$ unless $j = 1$; and $P(K, 0) = 1 = P(1, 1)$. Continuity of $R(\cdot)$ follows directly from the formulas, because $R(q)$ is a polynomial in q. To show that $R(\cdot)$ is increasing, first verify that

$$R(q) = \sum_{j=2}^{K} (s_{j-1} - s_j) \sum_{m<j} P(m,q),$$

using $s_K = 0$. Then observe that $\Sigma_{m<j} P(m,q)$ denotes the probability that more than $K-j$ other candidates

have made offers in an interval of probability q and that this probability must be a strictly increasing function of q. The ordering of the s_j values guarantees that at least one term in this $R(q)$ expression must have a positive $(s_{j-1} - s_j)$ coefficient and that none can be negative.

Next, we show that the lowest permissible offer 0 must be in the support of the equilibrium distribution of offers. The essential idea is that if the minimum of the support were strictly greater than zero, a candidate would be devoting positive resources to voters near to the minimum of the support of the distribution. He would expect to get almost no votes ($s_K = 0$) from these voters, because all other candidates would almost surely be promising them more. Thus, it would be better to reduce the offers to 0 for most of these voters in order to make a serious bid for at least some of their votes.

This argument can be formalized as follows. Because there are no points of positive probability, the cumulative distribution $F(\cdot)$ is continuous. Let z denote the minimum of the support of the equilibrium distribution, so that $F(z) = 0$ but $F(z + \varepsilon) > 0$ for all positive ε. Now, select any fixed y such that $y > z$ and $F(y) > 0$. For any ε such that $0 < \varepsilon < y - z$, a candidate might consider deviating from equilibrium by promising either y or 0 to each voter in the block of voters whom the candidate was supposed to offer between z and $z + \varepsilon$, according to the candidates F-distributed random-offer generator. These voters in this block were going to be given offers that averaged some amount between z and $z + \varepsilon$, so the candidate can offer y dollars to at least a z/y fraction of these voters without changing offers to any other voters. Among this z/y fraction of the block, the candidate would get an average point score of $R(F(y))$ by outbidding the other candidates who are using the F distribution; so the deviation would get him an average point score of at least $(z/y)R(F(y))$ from this block of voters. (The voters moved down to zero in this deviation would give the candidate $s_K = 0$ points.) The candidate who follows the equilibrium, however, gets at most $R(F(z + \varepsilon))$ as an average point score from this block of voters. Therefore, to deter such a deviation, we must have $(z/y)R(F(y)) \leq R(F(z + \varepsilon))$; and so $z \leq yR(F(z + \varepsilon))/R(F(y))$. But $R(F(z + \varepsilon))$ goes to $R(F(z)) = R(0) = 0$ as ε goes to 0, and so z must equal 0.

Now, let x and y be any two numbers in the support of the equilibrium distribution such that $0 < x < y$. A candidate could deviate by taking a block of voters to whom he or she is supposed to give offers close to x, according to the equilibrium plan and could, instead, give offers close to y to an x/y fraction of this block and could offer 0 to the remaining $(1 - x/y)$ fraction. Because the support of the distribution contains 0, as well as x and y, neither this self-financing deviation nor its reverse (offering close to x to a block of voters, of whom an x/y fraction were supposed get close to y and the remaining $(1 - x/y)$ fraction were supposed to get close to 0) should increase the candidate's expected average point score from this block of voters. Thus, we must have

$$R(F(x)) = (x/y)R(F(y)) + (1 - x/y)R(F(0)) .$$

But $R(F(0)) = R(0) = 0$, so $R(F(x))/x = R(F(y))/y$ for all x and y in the support of the equilibrium distribution. So there is some positive constant α such that for all x in the support of the distribution, $R(F(x)) = \alpha x$. The maximum of the support, where $F(x) = 1$, must be where $\alpha x = R(1) = 1$; so the maximum of the support must be at $1/\alpha$.

The support is the region where the cumulative distribution function is increasing and F has no discontinuous jumps, so $F(x)$ can equal the strictly increasing function $R^{-1}(\alpha x)$ on the entire support only if the support is the connected interval from 0 to the upper bound $1/\alpha$, with no breaks in between. (The inverse function $R^{-1}(\cdot)$ is well defined and strictly increasing on [0, 1] because $R(\cdot)$ is a strictly increasing function from [0, 1] onto [0, 1].)

To evaluate the constant α, we use the fact that the mean offer must equal 1 under the F distribution, so $\int_0^{1/\alpha} x\, dF(x) = 1$. We also know that a candidate who uses the same offer distribution F as all the other candidates must expect the average point score $\bar{s}$, so

$$\bar{s} = \int_0^{1/\alpha} R(F(x))\, dF(x) = \int_0^{1/\alpha} \alpha x\, dF(x) = \alpha .$$

Thus, the support of the F-distribution is the interval from 0 to $1/\alpha = 1/\bar{s}$, and the cumulative distribution satisfies the formula $R(F(x)) = \bar{s}x,\ \forall x \in [0, 1/\bar{s}]$.

It remains only to verify that we do get an equilibrium when F satisfies this formula. In general, for any nonnegative x, we have $R(F(x)) \leq \bar{s}x$, because $R(F(x)) = R(1) = 1 < \bar{s}x$ when $x > 1/\bar{s}$. So using any other distribution G that has mean 1 and is on the nonnegative numbers would give a candidate an expected score

$$\int_0^{\infty} R(F(x))\, dG(x) \leq \int_0^{\infty} \bar{s}x\, dG(x) = \bar{s},$$

with equality if the support of G is contained in the interval $[0, 1/\bar{s}]$. Thus, no candidate can increase his expected score by deviating from F to some other distribution, when all other candidates are using the distribution F. Q.E.D.

Implications. As a simple measure of potential inequality, the *maximal offer* may be defined as the highest offer in the support of a symmetric equilibrium offer distribution, for any given electoral system with any given number of candidates. By Theorem 2, this maximal offer is just the reciprocal of $\bar{s}$, the average of the ranking points, for any rank-scoring rule. Given any K, this average $\bar{s}$ is smallest under plurality voting, in which each voter gets a single nontransferable vote, because we then have $s_j = 0$ for all $j > 1$.

Thus, for any given number of candidates, plural-

TABLE 1

Standard Deviations of the Equilibrium Offer Distributions

NUMBER OF CANDIDATES	NUMBER OF NONCUMULATIVE VOTES THAT EACH VOTER MUST CAST						
	1	2	3	4	5	6	7
2	.58	—	—	—	—	—	—
3	.89	.45	—	—	—	—	—
4	1.13	.70	.38	—	—	—	—
5	1.33	.89	.59	.33	—	—	—
6	1.51	1.04	.75	.52	.30	—	—
7	1.66	1.18	.89	.67	.47	.28	—
8	1.81	1.31	1.01	.79	.60	.44	.26

Note: Each voter must cast a given number of noncumulative votes.

ity voting generates the most inequality of offers in a symmetric equilibrium, in the sense of maximizing the maximal offer that candidates give any voters. (Of course, we should not necessarily assume that the number of candidates for an office would remain constant when the electoral system is changed.) For plurality voting, $R(q) = P(1, q) = q^{K-1}$ and $\bar{s} = 1/K$. So the equilibrium cumulative distribution satisfies

$$x = (F(x))^{K-1}/(1/K) = K(F(x))^{K-1},$$

$$\forall x \in [0, 1/(1/K)] = [0, K],$$

and so

$$F(x) = (x/K)^{1/(K-1)}, \ \forall x \in [0, K).$$

When there are four candidates under plurality voting, each candidate offers less than .50 to half of the voters and offers less than 1 to 65% of the voters, but makes offers more than 2 to 20% of the voters, with some voters getting offers as large as 4. When the number of candidates increases to 10, each candidate offers less than .02 to half of the voters and favors a 16% minority with high offers above 2, some of whom get as much as the maximal offer 10. In general, the symmetric equilibrium under plurality voting has a maximal offer of K that is larger than the median offer of $K(.5^{K-1})$ by a factor of 2^{K-1}.

Each candidate i knows that when the other $K - 1$ candidates use the equilibrium distribution for plurality rule, the probability of the candidate's being ranked first by a voter to whom he or she offers x is

$$F(x)^{K-1} = ((x/K)^{1/(K-1)})^{K-1} = x/K.$$

That is, the highest of the other $K - 1$ candidates' offers, which candidate i must exceed in order to win the voter's vote, has a uniform distribution over the interval from 0 to K in equilibrium. Because of this uniform distribution, adding an extra ε units of money to the offer of any one voter has as much chance of gaining that voter's vote, no matter what this original offer might have been (between 0 and $K - \varepsilon$). Thus, there is no incentive for the candidate to revise the offers by taking from one voter and giving to another.

Among rank-scoring rules, for any given number of candidates K, the lowest possible maximal offer is $K/(K - 1)$, which is achieved by negative-plurality voting, where $s_j = 1$ for all $j < K$. In this sense, negative-plurality voting gives us the most egalitarian offer distributions. In equilibrium under negative-plurality voting, no voter is offered more than 1.34 when there are 4 candidates, and no voter is offered more than 1.12 when there are 10 candidates.

Following the insightful analysis of Cox (1987), we may characterize negative-plurality voting as a *last-place punishing* scheme, whereas (ordinary) plurality voting is a *first-place rewarding* scheme. Under negative-plurality voting, each candidate wants to minimize the fraction of the voters who think that he or she is the worst candidate, so candidates try to avoid offending many voters. Each candidate makes low offers close to 0 to a small minority of voters, in order to finance competitive offers above 1 to the majority, but the number of such neglected voters is kept small. For negative-plurality voting, the equation in Theorem 2 becomes

$$x = (1 - (1 - F(x))^{K-1})/((K - 1)/K).$$

This formula implies, for example, that each candidate offers less than 1 to only 37% of the voters when there are 4 candidates and less than 1 to only 23% of the voters when there are 10 candidates.

Under a voting rule in which each voter must vote for exactly V of the K candidates, the maximal offer is K/V, because $\bar{s} = V/K$. Using the general formula in Theorem 2 to compute the inverse of the cumulative distribution function, standard deviations of the equilibrium offer distributions can also be calculated. Table 1 shows such these standard deviations for voting systems with V noncumulative votes and K candidates. Notice that, like the maximal offer K/V, these standard deviations increase as K increases and as V decreases. In each row of Table 1, the leftmost entry is for plurality voting and the rightmost entry is for negative-plurality voting. Figures 1–3 show the probability densities of the equilibrium offer distributions, when there are four candidates, for the cases of $V = 1$ (plurality), $V = 2$, and $V = 3$ (negative plurality).

Borda voting is another important rank-scoring rule, in which $s_j = (K - j)/(K - 1)$. So $\bar{s} = 1/2$, and the maximal offer is 2. The function $R(F(x))$ under Borda rule is the expected value of $(K - j)/(K - 1)$, where j is a random variable such that $K - j$ has a binomial distribution with expected value $(K - 1)F(x)$. Thus, by Theorem 2,

$$x/2 = \bar{s}x = R(F(x)) = \sum_{j=1}^{K} P(j, F(x))(K - j)/(K - 1)$$

$$= (K - 1)F(x)/(K - 1) = F(x), \quad \forall x \in [0, 2].$$

Thus for any number of candidates K, the equilibrium offer distribution under Borda rule is a uniform distribution over the interval from 0 to 2. The standard deviation of this distribution is approximately .58.

For a voting rule that has the same maximal offer as Borda but that has a tighter distribution as measured by the standard deviation, consider the rank-scoring rule in which $s_1 = 1$, $s_j = 1/2$ for all j such that $1 < j < K$, and $s_K = 0$. (This rule is equivalent to giving each voter one positive vote and one negative vote.) Figure 4 shows the offer distribution for this rule when $K = 4$. The maximal offer under this voting rule is also equal to 2 for all K, because $\bar{s} = 1/2$, but the equilibrium offer distribution has standard deviation .52 when $K = 4$, and .37 when $K = 8$.

As we compare these equilibrium offer distributions, the fact that plurality voting generates much greater inequality among voters than other electoral systems may be quite disturbing. In defence of plurality voting, one may argue that our symmetry assumption is unlikely to be satisfied in plurality elections with many candidates. Duverger's law (see Riker 1982) predicts that in multicandidate races under plurality voting, the voters will take only two candidates seriously and that each voter will vote for the candidate whom the voter prefers among these two serious candidates (even if there are other candidates whom the voter likes even better). That is, the symmetric equilibrium in which every candidate is considered to have a serious chance of winning is much less likely to be observed than one of the nonsymmetric equilibria in which only two candidates are considered to have a substantial chance of winning. By showing the problematic nature of the symmetric equilibrium that Duverger's law excludes, the analysis here casts a rather favorable light on Duverger's law. If Duverger's law did not hold, then the use of plurality voting in multicandidate winner-take-all elections would more often give us elected leaders who deliberately appeal to only a small minority of the voters.

APPROVAL VOTING

Under *approval voting*, each voter indicates approval of a set of candidates on the ballot. A voter is allowed to approve of any number of candidates. Each candidate's approval score is then the fraction of the voters who have approved of the candidate. That is, each voter can give each candidate either an approval (worth 1 point) or a disapproval (worth 0 points), and the winner is the candidate who gets highest average point score from the voters. Approval voting is a scoring rule but it is not a rank-scoring rule, because the point scores of various candidates cannot be determined simply from a voter's rank-ordering of the candidates. For example, when there are three candidates, one voter might give an approval vote only to one favorite candidate, whereas another voter with the same rank-ordering of candidates might give an approval vote to the top two favorites.

We need to formulate a voter's decision criterion in order to describe how each voter would decide how many candidates to approve in a symmetric equilibrium under approval voting. Let us number the K candidates 1, 2, . . . , K. Now consider a voter and let

FIGURE 1

Offer Distribution with One Vote, Four Candidates

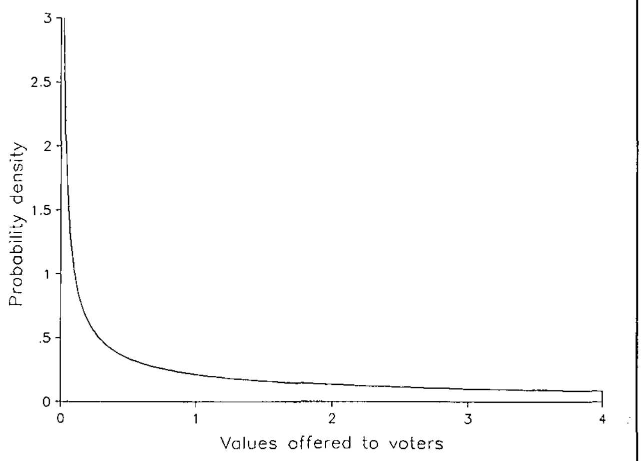


FIGURE 2

Offer Distribution with Two Votes, Four Candidates

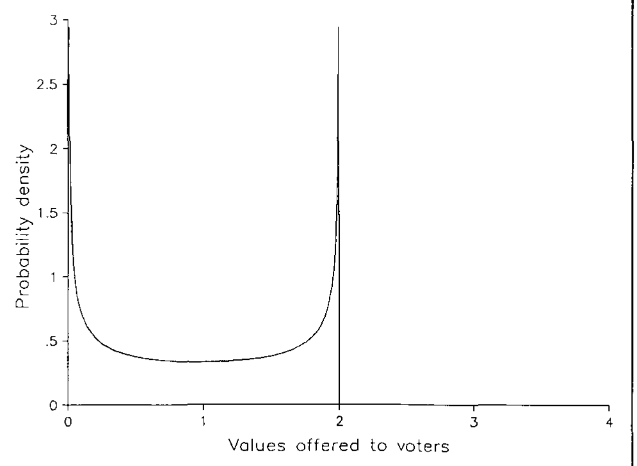
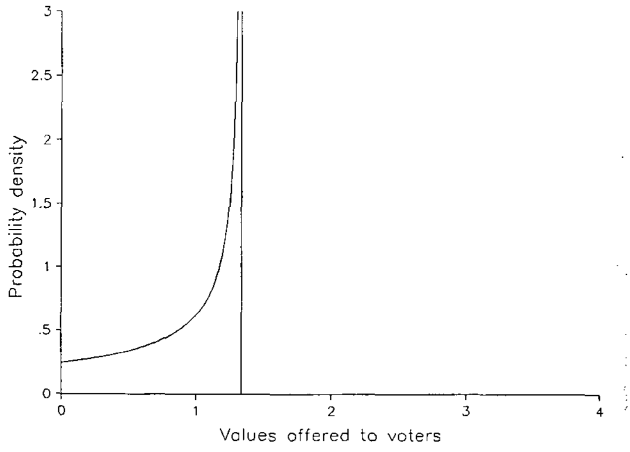


FIGURE 3

Offer Distribution with Three Votes, Four Candidates



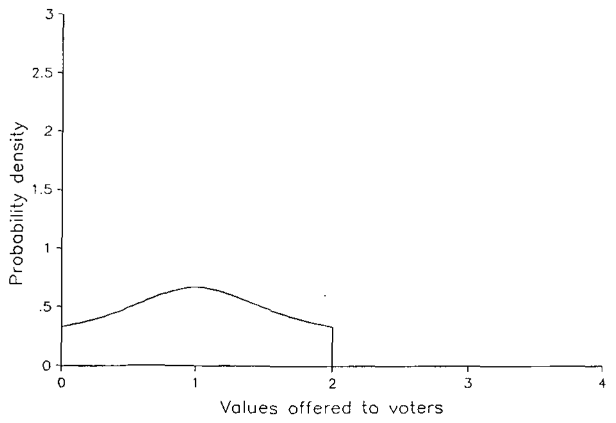
u_i denote the value of the offer that candidate i has promised to this voter. The voter's decision to give an approval vote to candidate i will only matter if candidate i is in a close race with some other candidate h (where h would win unless this voter adds an approval vote for i). In such a case, giving an approval vote to candidate i would change the voter's payoff from u_h to u_i. Our symmetry assumption implies that in the case of such a close race involving candidate i, any of the other $K - 1$ candidates is equally likely to be the candidate h who is in a close race with candidate i. Thus, the expected net gain for the voter from giving an approval vote to candidate i is proportional to

$$\sum_{h \neq i}(u_i - u_h)/(K - 1) = (u_i - \bar{u})(K/(K - 1)),$$

where $\bar{u} = \Sigma_{h=1}^{K} u_h/K$. Thus, in a symmetric equilibrium, each voter should compute the average $\bar{u}$ of the offers that the K candidates have promised, give an approval vote to every candidate i such that $u_i > \bar{u}$, and not approve of any candidate who has offered less than this average $\bar{u}$.

FIGURE 4

Offer Distribution with Ranking Points (1, .5, .5, 0)



Given any m candidates who are generating their offers independently according to the cumulative distribution function $F(\cdot)$, let $A_m(x)$ denote the probability that the average of their m offers is less than x, for any given voter.[4] Now, suppose that some candidate i offers an amount u_i to some voter, while the other $K - 1$ candidates are selecting their offers independently according to the cumulative distribution function F. Then the probability of candidate i getting an approval vote from this voter is $A_{K-1}(u_i)$, because candidate i's offer u_i is greater than the average of all K offers if and only if u_i is greater than the average of the other $K - 1$ candidates' offers.

To win, a candidate wants both to increase his or her own score and decrease the scores of opponents. Under approval voting, increasing the vote score for candidate i is not the same thing as decreasing the vote scores of the other candidates, because the total number of points that each voter gives is not fixed under approval voting (unlike the rank-scoring rules already considered). For any candidate h other than i, let $B(u_i)$ denote the probability that candidate h will get an approval vote from any given voter, when candidate i offers u_i and all other candidates' offers are drawn independently from the F-distribution.[5]

Candidate i would gain by deviating from the F-distribution to some other distribution G (when all other candidates are expected to use the F-distribution) if the deviation would cause candidate i's average approval score to become higher than the other candidates' average approval scores. I am assuming here that the number of voters is large or effectively infinite, so that a candidate's average approval score is equal to the probability of any one voter giving the candidate an approval vote. Thus there is an *equilibrium* in which all candidates generate offers according to the F distribution if and only if

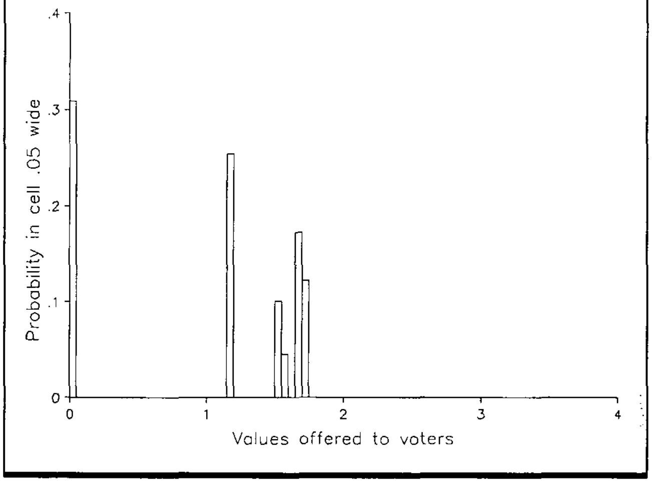
$$\int_0^\infty A_{K-1}(x)\, dG(x) \leq \int_0^\infty B(x)\, dG(x)$$

for every nonnegative distribution G with mean 1. (Unwinding the definitions, it can be shown that this inequality becomes an equality when $G = F$.)

It seems quite difficult explicitly to calculate equilibrium offer distributions for approval voting.[6] I have analyzed approximate solutions for a discrete version of this problem, in which all offers are integer multiples of .05. The resulting equilibrium probability distribution for the case of 4 candidates is shown in Figure 5. For any number of candidates up to 10, Table 2 shows the standard deviation of the equilibrium offer distribution, the maximal offer, and the average approval score for each candidate, in the symmetric equilibria that have been found for this .05-discrete approximation. In each case, I have found only one equilibrium for each number of

FIGURE 5

Offer Distribution with Approval Voting, Four Candidates



candidates. The convergence of the algorithm seemed sufficiently robust to justify a conjecture that this symmetric equilibrium might be unique, but I cannot prove that there is a unique symmetric equilibrium under approval voting.

As Table 2 shows, equilibrium offer distributions for approval voting have maximal offers that are between 1 and 2, and these maximal offers decrease as the number of candidates increases. As Figure 5 illustrates, the support of the equilibrium offer distribution is not convex when $K \geq 3$. Instead, each candidate gives offers that are greater than 1 to a majority of the voters, while the remaining voters (a minority that decreases in size as K increases) get offers that are close to 0.

To get some intuition for these results, suppose that the number of candidates K is large and consider a nonequilibrium scenario in which all candidates draw their offers from the uniform distribution on 0 to 2. The central limit theorem tells us that the average of any $K - 1$ candidates' offers to any given voter has a distribution that is approximately normal, with mean 1 and a small standard deviation. The probability that a candidate would gain (or lose) a voter's approval by slightly increasing (or decreasing) this voter's offer is proportional to the probability density of the average of the other $K - 1$ candidates' offers, and the normal approximation has a density that is highest at 1. So a candidate could increase the expected approval score by decreasing offers that are far from 1, and then using the resources thus made available to correspondingly increase offers that are close to 1. The result of such transfers would be to move toward a distribution in which some voters get offers near 0 and the rest of the voters get offers that are larger than 1 but less than 2. Such a distribution is in fact shown in Figure 5, where about 31% of the voters get offers close to 0, 25% of the voters get offers close to 1.15, 15% of the voters get offers close to 1.50, and 29% of the voters get offers close to 1.70. It can be shown that, like the rank-scoring rules, the equilibrium offer distribution under approval voting must be continuous, even though the support of the distribution is not convex.

TABLE 2

Candidate Offers in Equilibrium under Approval Voting

NUMBER OF CANDIDATES	S.D.	MAX. OFFER	FRACTION < 1	AVG. SCORE
2	.58	2.00	.50	.50
3	.71	1.90	.34	.55
4	.70	1.70	.31	.60
5	.66	1.60	.29	.63
6	.65	1.50	.29	.66
7	.61	1.45	.27	.69
8	.61	1.40	.27	.71
9	.56	1.35	.24	.74
10	.54	1.35	.23	.74

Note: Statistics were calculated using a .05 discrete approximation.

SINGLE TRANSFERABLE VOTE

Let us consider now a simple version of *single transferable vote* (STV), in which K candidates are competing for a single office. (Multiseat councils will be considered later.) Each voter submits a ballot that ranks the K candidates in some order, chosen by the voter. These ballots are then recounted a sequence of times until a candidate wins. In the first count, each voter's ballot is counted as a vote for the candidate who is listed at the top of the voter's rank ordering. If no one candidate gets a majority in this count, then the candidate with the lowest share of the votes is eliminated from consideration, and each ballot that was assigned to this candidate is reassigned to the candidate who is ranked highest on the ballot among those who have not yet been eliminated. Then the ballots are recounted, and this process is repeated until some candidate gets a majority. When a candidate gets a majority of the votes in a count, then that candidate wins the election. Thus, STV is a ranking rule in the sense that each voter's ballot can be written as a rank ordering of the candidates, but it is not a scoring rule, because a voter's rank ordering does not simply generate one point score for each candidate.

Our symmetry assumption implies that voters will vote sincerely under STV (because a voter will not perceive any pair of candidates as being more likely than any other pair to be in a close race where the voter's vote could make a difference in the outcome). That is, we may assume here that each voter will rank the candidates in order of their offers, putting the candidate who has offered the most at the top of his rank ordering.

In a symmetric equilibrium of our model, we should expect the candidates to divide the votes equally at every round. Thus we need to be careful about the interpretation of ties. Remember that our model assumes that the number of voters is infinite, but this assumption is meant only as an approximation to a large finite population. When two or more candidates get the same share of the votes in our model, we should understand that this means that the difference in their vote totals is a vanishingly small or infinitesimal fraction of the overall population, but (because the population is very large) there may still be a difference of hundreds of votes between the absolute vote totals of these candidates. Thus, a tie for last place in our infinite-population model should be interpreted as an outcome in which exactly one of these candidates is actually the lowest. By symmetry of the scenario, each of the apparently tied candidates is equally likely to be the actual last-place candidate who then is eliminated.

When all candidates independently generate offers from the same cumulative distribution F, there will be a symmetric allocation of votes. Thus no candidate will ever get a majority until there have been $K - 1$ counts, at the last of which all but two candidates will have been eliminated (so each will get approximately a 50% share, which will really be a strict majority for one of the two candidates). At the first count, each candidate will have a probability $1/K$ of being eliminated and a probability $(K - 1)/K$ of being retained for the second count. At the second count, each remaining candidate will have a probability $1/(K - 1)$ of being eliminated and a probability $(K - 2)/(K - 1)$ of being retained; and so on. Each candidate's probability of winning is thus

$$((K-1)/K)((K-2)/(K-1)) \ldots (1/2) = 1/K.$$

Now suppose that some candidate i deviates from this symmetric scenario and generates offers according to some other cumulative distribution G, while all other candidates continue to use the F-distribution. Consider a count in which candidate i is one of n candidates who have not yet been eliminated. A voter who has been offered u_i by candidate i will rank candidate i at the top of the list (when the eliminated candidates are ignored) if candidate i offered more than the other $n - 1$ candidates, which has probability $F(u_i)^{n-1}$. So candidate i's vote share at this count will be $\int_0^\infty (F(u_i))^{n-1}\, dG(u_i)$. If this vote share is greater than the average $1/n$, then candidate i will surely not be eliminated at this count. On the other hand, if this vote share at this round is less than $1/n$, then candidate i must be in last place because the other candidates are symmetric in this scenario, and so candidate i will surely be eliminated at this count.[7]

Being strictly higher than everyone else in the early counts is irrelevant if elimination is certain at any one of the later counts. So a deviating candidate's probability of winning will go to zero if the candidate's expected vote share is less than the average at any of the $K - 1$ counts of the ballots. Thus, we may say that there is an *equilibrium* in which all candidates use the F-distribution if and only if there does not exist any probability distribution G on the nonnegative real numbers, with mean 1, such that

$$\int_0^\infty (F(x))^{n-1}\, dG(x) \geq 1/n,$$

for every n in $\{2, 3, \ldots, K\}$, and

$$\int_0^\infty (F(x))^{m-1}\, dG(x) > 1/m,$$

for at least one number m in $\{2, 3, \ldots, K\}$.

We can now show that there are multiple symmetric equilibria under STV. These equilibria include offer distributions that are arbitrarily close to any of the equilibria that we got for plurality voting when the number of candidates was between two and K. Thus, any number between two and K can be the maximum of a symmetric equilibrium offer distribution under STV. The intuition behind this multiple-equilibrium result is simple. To gain from deviating from a symmetric scenario, a candidate must avoid losing in $K - 1$ counts. Thus an equilibrium can be supported by any profile of campaign strategies that deters such deviations in at least one of these $K - 1$ counts.

THEOREM 3. *Let $(\lambda_2, \ldots, \lambda_{K-1}, \lambda_K)$ be any vector such that*

$$\lambda_n > 0, \ \forall n \in \{2, \ldots, K-1, K\}$$

and $\Sigma_{m=2}^{K} \lambda_m = 1$. Let F be defined by the equation $x = \Sigma_{n=2}^{K} n\lambda_n(F(x))^{n-1}$, $\forall x \in [0, \Sigma_{m=2}^{K} m\lambda_m]$. Then there is an equilibrium under single transferable vote in which all candidates generate offers according to the F-distribution.

Proof. Notice first that the function F described in the theorem always exists, because the equation directly defines its inverse

$$F^{-1}(q) = \sum_{n=2}^{K} n\lambda_n q^{n-1}, \ \forall q \in [0, 1].$$

Then F is well defined because F^{-1} is strictly increasing and thus one-to-one on its range. To verify that the mean is 1, observe that

$$\int_0^\infty x\, dF(x) = \int_0^1 F^{-1}(q)\, dq = \int_0^1 \left(\sum_{n=2}^{K} n\lambda_n q^{n-1} \right) dq$$

$$= \sum_{n=2}^{K} n\lambda_n \left(\int_0^1 q^{n-1}\, dq \right) = \sum_{n=2}^{K} n\lambda_n (1/n) = 1 .$$

If the F-distribution did not form an equilibrium, then there would exist an offer distribution G such that

$$\sum_{n=2}^{K} \lambda_n \left(\int_0^{\infty} (F(x))^{n-1}\, dG(x) \right) n > 1,$$

because the coefficient of λ_n would be at least 1 in each term, with at least one strict inequality. Then, by the definition of F, we would get

$$1 < \int_0^{\infty} \left(\sum_{n=2}^{K} n\lambda_n (F(x))^{n-1} \right) dG(x) = \int_0^{\infty} x\, dG(x),$$

and so the deviation G must have a mean greater than 1, which is not permissible for a candidate. Q.E.D

In the extreme case where some λ_n coefficient is close to 1 and all other λ_m are close to 0, the equation in Theorem 3 that defines F becomes close to $x = n(F(x))^{n-1}$, which characterizes the symmetric equilibrium offer distribution for plurality voting with n candidates.

MULTISEAT ELECTIONS

I have so far only discussed the case of individual candidates competing for a single office, but most of our results can be readily extended to competition for multiple seats and to competition among parties. The crucial defining property of the symmetric equilibria that we have studied is that no candidate can break the symmetry in his or her own favor (scoring higher than the still-symmetric other candidates) by deviating from the equilibrium strategy. This characterization of symmetric equilibria remains the same when there are many seats at stake, as long as the number of candidates is greater than the number of seats.

For example, consider an election in which K candidates are competing for L seats in a council, where $1 \leq L \leq K - 1$ and a rank-scoring rule is used with ranking points $(s_1, s_2, \ldots, s_K)$ such that $1 = s_1 \geq s_2 \geq \ldots \geq s_K = 0$. The candidates with the L-highest scores will win the L seats. Suppose, for simplicity, that each candidate who wins a seat in the council will control a portion of the government budget that has an expected average value of one dollar per voter. Each winner will allocate his or her portion of the government budget to the voters according to the campaign promises that the candidate independently generated from his or her offer distribution. (A more complicated story might say that only a member of the council who is a member of a governing majority coalition that will take control of the council after the election will get a share of the budget. When the candidates are symmetric, however, we may assume that any candidate who wins a seat will be equally likely to be included in the governing coalition, and so each candidate who wins has the same expected budget to allocate.)

Under these assumptions, the symmetric equilibrium offer distribution, for any given rank-scoring rule, is exactly the same as described in Theorem 2. In this multiseat case, each candidate has a probability L/K of winning in the symmetric equilibrium, but (as in the one-seat case) he would still deviate only if doing so would raise expected score above the other still-equal (by symmetry) scores of the other $K - 1$ candidates. So the conditions for a symmetric equilibrium with K candidates depend only on the ranking points $(s_1, s_2, \ldots, s_K)$ but do not depend on the number of seats L, as long as $L < K$. Thus, our results for the one-seat case in Theorem 2 can be immediately generalized to multiple seats. For example, the rank-scoring rule with the ranking-points vector $(1, 0, \ldots, 0)$ is called SNTV when there are multiple seats at stake. So the symmetric equilibrium offer distribution for SNTV with K candidates and L seats is the same as the symmetric equilibrium offer distribution that we found for plurality voting with K candidates in the one-seat case.

Similarly, the equilibrium offer distributions that I described for approval voting with K candidates and one seat apply equally well when the K candidates are competing for L seats and the winners will be the candidates with the L-highest approval scores.

Under single transferable vote, we do find some difference in the case of multiple seats. With L seats, the majority quota is replaced by a $1/(L + 1)$ quota and the recounting stops after the count in which one candidate is eliminated out of $L + 1$ remaining. Thus, we need to revise Theorem 3 by changing the number 2 to $L + 1$ throughout, but then the revised theorem can be applied to the case where L seats are being allocated under STV. So when K candidates compete for L seats under STV, there are multiple symmetric equilibria in which the maximal offers range from $L + 1$ up to K.

The most substantial difference that we may find in the multiseat case is that the assumption of a symmetric multicandidate equilibrium may become more compelling. In single-seat plurality elections, Duverger's law predicts that two candidates will be distinguished as the only two serious contenders. In a multiseat election under SNTV, however, we should expect that there will be more serious candidates than seats (see Cox 1992; Reed 1990). Thus, with multiple seats, we cannot use Duverger's law to escape from the fact that SNTV may encourage candidates to make extreme promises to narrow subsets of the electorate.

Multiseat districts in Japan use SNTV, and it is also used in intraparty competition in some open-list proportional representations systems, as in Brazil. In open-list proportional representation systems, when a party nominates more candidates than the number of seats that it wins, the nominated candidates who actually get the party's seats are determined by voters' votes for the individual candidates. To be specific, suppose that a party has nominated K candidates but that the party is expected to win only L seats and each voter who votes for the party can vote for one individual candidate on the ballot. The party's L seats will be won by those among its candidates who get the L-highest scores in this contest among the individual candidates. Then our analysis of plurality voting and SNTV with K candidates can be directly applied to the race among the K nominees of this party for its L seats. Our results suggest that the K nominees will make highly unequal offers to voters and that each will concentrate attention on a small minority of the voters who support the party. This prediction is consistent with the bailiwick phenomena (in which candidates seek votes only in very narrow subsets of the electoral district) that Ames (1992) has described in Brazilian legislative elections. The model predicts that the candidates could be induced substantially to broaden their appeal if the individual candidates in party lists were ranked by another voting system, such as Borda or multiple noncumulative votes or approval voting.

The analysis can also be applied to the competition among parties in proportional representation systems. For simplicity, suppose that each party may expect to control a portion of the government budget that is proportional to the share of the legislature that it wins. Suppose also that each party will allocate its portion of the budget to individual voters in proportion to its campaign promises, which it has independently generated for every voter according to its offer distribution. Each voter must choose one party to support, and each party gets seats in proportion to its share of the vote. So each voter should support the party that has promised that voter the most.

Under these assumptions, our analysis of proportional representation looks like that of plurality voting and SNTV, because proportional representation uses the same ranking-points vector $(1, 0, \ldots, 0)$ and thus is also a pure first-place rewarding scheme. The symmetric-equilibrium offer distributions in a proportional representation election among K parties will be the same as the symmetric-equilibrium offer distributions in a one-seat plurality election among K candidates. Thus, in a proportional representation system, if many parties are perceived as having similar potential to participate in the process of forming a government, each of these parties may feel compelled to appeal narrowly to a small group of voters, and their campaign promises may tend to create special interest divisions even in an otherwise homogeneous electorate.

MINORITY REPRESENTATION

I have focused on only one attribute of electoral systems: the degree to which they may encourage candidates to create inequalities among the voters by favoring some voters at the expense of others. For a full comparison of electoral systems, we must consider other models emphasizing other attributes of electoral systems. One other attribute that I should at least briefly consider here, because of its close relationship to my main topic, is the ability of electoral systems to guarantee diverse representation of minority groups. I follow here the seminal analysis of this question by Cox (1987, 1990).

Consider again an election in which K candidates are competing for election to a council of L members and people vote according to a rank-scoring rule with ranking points $(s_1, s_2, \ldots, s_K)$ such that $1 = s_1 \geq s_2 \geq \ldots \geq s_K = 0$. I assume that $L \leq K - 1$ and that the candidates with the L-highest scores will win the L seats. Now suppose that instead of making a discretionary allocation of a given budget (as I have assumed up until now), government officials have only one simple yes-or-no policy question to decide (e.g., "Should our nation ratify the new regional trade agreement?"). At the beginning of the campaign, each candidate must choose to be either on the affirmative side or on the negative side of this policy question. Every voter has preferences one way or the other on this question and will select a vote to try to maximize the number of elected officials who have chosen the policy answer that the voter prefers. As usual, we suppose that the number of voters is very large (or is an infinite measure space). Let Q denote the fraction of the voters who prefer the affirmative answer to the policy question and suppose that Q is common knowledge.

How large must the affirmative fraction Q be to guarantee that there does not exist any symmetric equilibrium in which all of candidates are expected to choose the negative side of the policy question? That is, what is the largest minority group that may be ignored by all candidates in the election, when each candidate chooses a policy position so as to maximize the probability of winning a seat? This upper bound for the size of a voting block that all candidates could ignore may be called the *Cox threshold.*

If all candidates choose the negative side of the policy position, then (by symmetry) they must all expect the same average score $\bar{s} = \Sigma_{j=1}^{K} s_j/K$, and so each candidate has a probability L/K of winning a seat. If one candidate deviates to the affirmative side, while all other candidate stay on the negative side, then this deviating candidate will get a score $Qs_1 + (1 - Q)s_K = Q$, because the affirmative voters will rank the candidate at the top of their ballots, while the negative voters will rank the candidate at the bottom of their ballots. The symmetry assumption implies that the other $K - 1$ candidates who are all on the negative side will get the same expected score, and we know that the scores of all K candidates must average to $\bar{s}$. So the deviating candidate cannot expect to win if $Q < \bar{s}$, but the single deviating candidate should expect to win a seat if $Q > \bar{s}$. (The large-population assumption is used here only to justify the assumption that the candidate with the highest expected score will almost surely have the highest realized score.) Therefore, to guarantee that there is no symmetric equilibrium in which the affirmative block is ignored by all K candidates, the affirmative block must be greater than an $\bar{s}$ fraction of the electorate. Thus, as Cox (1987, 1990) has shown, the Cox threshold for a rank-scoring rule is $\bar{s}$.

Of course, guaranteeing that some candidates will offer to represent the affirmative block of voters does not guarantee that the affirmative block will actually win representation in the council except in the case of $L = K - 1$. When there are seats for all except one candidate, then a lone candidate on one side of the policy question will fail to get a seat only if that candidate is the lowest scorer, which cannot happen if his or her score Q is above the average score $\bar{s}$. Thus a block of voters that is larger than $\bar{s}$ is sure to win at least one seat in equilibrium when $L = K - 1$, even without the symmetry assumption.

For any rank-scoring rule, the Cox threshold $\bar{s}$ is exactly the reciprocal of the maximal offer that we found in Theorem 2. In this sense, the rank-scoring rules that are best for guaranteeing representation of small minorities in large multiseat elections are also the worst for deterring candidates from creating inequalities in a homogeneous population. That is, electoral provisions (like SNTV) that are designed to give representation to diverse minority groups may also have the effect of encouraging candidates to create special interest groups and minority conflict even when it would not otherwise exist. Designers of electoral systems should recognize a trade-off between these two desiderata: guaranteeing representation for minority groups in the electorate and encouraging individual candidates to appeal broadly to the electorate as a whole.

With respect to these two desiderata, however, there seems no reason to use a system that has a low Cox threshold (which encourages a diversity of narrowly focused candidates, rather than candidates who try to appeal broadly to the whole electorate) unless the number of seats being filled is large enough for this diversity of candidates to be translated into a diversity of elected officials. In a single-seat election with two policy alternatives, for example, the minority cannot be represented by a winning candidate unless the majority is left unrepresented after the election.

To guarantee that clear majority interests are not neglected by all candidates in equilibrium, a good electoral system should have a Cox threshold that is not greater than 1/2. For example, negative-plurality voting with 10 candidates would give us a Cox threshold of $\bar{s} = .90$. Thus even if 89% of the voters preferred the affirmative position in my simple model, there would exist a symmetric equilibrium in which all 10 candidates chose the negative policy position. One candidate who deviated alone to the affirmative side would get a zero rating from 11% of the voters, while the nine negative candidates would each expect to get zero ratings from $.89/9 = 9.89\%$ of the voters, and so the affirmative candidate would lose. Of course, there exists another equilibrium in which all candidates coordinate on the majority side. However, the danger of having an equilibrium in which an overwhelming majority is so completely unrepresented makes negative plurality a very undesirable system. This danger of neglecting majority interests similarly leads us to reject all rank-scoring rules that have $1/\bar{s} < 2$, even though, by Theorem 2, such rules may seem good for reducing the inequity of candidates' offer distributions.

The exact reciprocal relationship between the Cox threshold and the maximal offer that we have found for rank-scoring rules does not necessarily apply to other electoral systems. Under STV with K candidates and L seats, the Cox threshold is $1/(L + 1)$, because a candidate is guaranteed a seat when he is ranked at the top by more than $1/(L + 1)$ of the electorate. But Theorem 3 shows symmetric equilibria in which some voters get offers that are arbitrarily close to K, which can be strictly greater than $L + 1$. Thus, we find maximal offers that are above the reciprocal of the Cox threshold for STV.

Under approval voting with any number of candidates and any number of seats, the Cox threshold is 1/2. If the candidates differed only in their positions on one binary policy question, then each voter would sincerely approve all candidates who adopted that voter's preferred position; and so each candidate would maximize his or her approval score by choosing the position that is favored by the majority (see Cox 1987; Myerson 1993). But Table 2 shows that for approval voting with more than two candidates, the maximal offers in symmetric equilibrium offer distributions are strictly less than 2. So we find maximal offers that are below the reciprocal of the Cox threshold for approval voting, and approval voting allows us to get maximal offers less than 2 without having a Cox threshold above 1/2.

Notes

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1. With a finite population of M voters and with a fixed supply of M dollar's worth of public resources to be allocated, campaign promises could not be independent across all voters, because the offers to all voters would have to sum to the given number M. However, as the number M increases, independently distributed offers can be more and more closely approximated. If the mean of the candidate's offer distribution is not more than 1 and the support of the distribution is bounded, then for any small positive number ε, M voters' offers that are drawn independently from the candidate's distribution would have probability less than ε of totaling more than $M(1 + \varepsilon)$ dollars, when M is sufficiently large. Thus, taking the limit as the population goes to infinity, we can assume that each candidate makes independent offers to every voter. Conversely, De Finetti's theorem in probability theory implies that if an infinite number of voters are treated identically by a candidate in the sense that their offers are exchangeable random variables, then their offers must be conditionally independent, given the candidate's offer distribution.

2. The Riemann–Stieltjes integral is a generalization of the Riemann integral, which allows us to express expected values with respect to both continuous and discrete probability distributions. Let $a(x)$ denote any bounded continuous function of the variable x and let $b(x)$ denote any bounded and nondecreasing function of x. Then the Riemann–Stieltjes integral $\int_0^\infty a(x)\, db(x)$ can be defined as the limit, as $n \to +\infty$, of the sum

$$\sum_{i=1}^{\infty} a(i/n)[b(i/n) - b((i-1)/n)].$$

If the function b is differentiable, then

$$\int_0^{\infty} a(x)\, db(x) = \int_0^{\infty} a(x)\, b'(x)\, dx.$$

3. The candidates' payoff functions are discontinuous when their expected scores are equal. Simon and Zame (1990) show that to find the equilibria of such discontinuous games, we may need to allow the payoffs to be endogenously redefined at points of discontinuity, requiring only that these endogenous payoffs should be selected in the convex-valued upper-hemicontinuous extension of the original payoff function. However, I am analyzing here only symmetric equilibria (which must exist, because the game is symmetric), and candidates who use the same offer distribution must have the same probability of winning. Thus the methodology of Simon and Zame leads us to use a simple model in which candidates with the same expected score have the same probability of winning.

4. For each $m \geq 1$, $A_m(\cdot)$ is the cumulative probability function for the average of m offers that are sampled independently from the F-distribution. These cumulative functions may be inductively calculated by the equations

$$A_1(x) = F(x),$$

$$A_m(x) = \int_0^{mx} A_{m-1}((mx - z)/(m-1))\, dF(z),\ \forall x.$$

To derive this formula, I use the fact that if y is the average of $m - 1$ candidates' offers and z is another candidate's offer, then the average of these m candidates' offers is $((m-1)y + z)/m$, which is less than x if and only if $y \leq (mx - z)/(m-1)$.

5. To formally define $B(u_i)$, consider any other candidate h, consider some specific voter, and let y denote the average of the offers to this voter from all candidates other than i and h. Then candidate h will get an approval vote from this voter if

$$u_h > (u_h + u_i + (K-2)y)/K.$$

So, assuming $K \geq 3$, candidate h will get an approval vote if

$$((K-1)u_h - u_i)/(K-2) > y\ .$$

Thus, the probability that candidate h will get an approval vote from this voter, when candidate i offers u_i and all other candidates' offers are drawn from the F-distribution, is

$$B(u_i) = \int_0^{\infty} A_{K-2}(((K-1)u_h - u_i)/(K-2))\, dF(u_h),$$

where $A_{K-2}(y) = 0$ if $y < 0$.

6. In an equilibrium under approval voting, no candidate i should be able to increase the average value of $A_{K-1}(u_i) - B(u_i)$ for some block of voters by transferring promised resources among these voters. So let x and y be numbers such that $0 < x < y$ and x is in the support of the F-distribution. Suppose that candidate i is to consider the following perturbation of the F-distribution: all voters who were supposed to be given an offer close to x (according to the random number generator that implements F) will instead be offered either 0 or y, and to make this perturbation self-financing, the fraction of these voters who will get y will be close to x/y. As a result of this perturbation, the net change in the average of candidate i's objective $(A_{K-1} - B)$, for this block of voters, would be

$$(x/y)(A_{K-1}(y) - B(y)) + (1 - x/y)(A_{K-1}(0) - B(0)) - (A_{K-1}(x) - B(x)) = (C(y) - C(x))x,$$

where the function $C(\cdot)$ is defined such that

$$C(z) = (A_{K-1}(z) - B(z) - A_{K-1}(0) + B(0))/z,\ \forall z > 0.$$

This gain must be nonpositive if x is in the support of the F-distribution, and it must also be nonnegative if y and 0 are in the support of the F-distribution (in which case this perturbation could be reversed). So we can construct an equilibrium offer distribution by choosing F so that the support of the F-distribution is in the set of numbers z that maximize the function $C(z)$. This C-maximization condition is difficult to solve analytically, because $C(\cdot)$ depends on $F(\cdot)$, but iterative methods can be used to find approximate solutions. The Pascal program that I have used to find approximate equilibrium offer distributions for approval voting is available on request.

7. There is some technical complexity in the case where the deviating candidate i's expected vote share exactly equals the average $1/n$ in an STV recount when n candidates remain. The definition of equilibrium used here is consistent with the simple assumption that when expected vote shares are equal, then each candidate is equally likely to be eliminated. However, if G is strictly different from F, we cannot be sure that an equal expected vote share in the infinite-population limit implies an equal probability of being last in the case of a large finite population.

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POLICY, INSTITUTIONS, AND COALITION AVOIDANCE: NORWEGIAN GOVERNMENTS, 1945–1990

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Norwegian party politics is characterized by coalition avoidance that defies conventional coalition theory. This failure of coalescence can be caused either by policy pursuit (preference-induced) or by institutional constraint (structure-induced). We test the explanatory power of policy-based and institutional explanations, relying on content analysis of authoritative party and government documents for our policy measures. The results show that the left–right policy dimension has powerfully constrained Norwegian interparty bargaining and that policy-based coalition theory can account for many apparent anomalies in Norwegian coalition politics. A permissive institutional environment has also fostered coalition avoidance. Although core-based coalition theory can thus be successfully adapted to the Norwegian case, it rests on a number of critical assumptions that limit its general applicability.

Elections are the principal instrument of popular control in representative democracy. Ideally, elections determine who governs and what public policies are conducted (King 1981). In the real world of democracy, however, the verdicts of the electorate require elaboration and interpretation. In multiparty parliamentary democracies, political parties play a powerful intermediary role in determining the composition of the executive branch as well as the content of public policy. Where no single majority exists, legislative parties must, under majority rule, build coalitions to effect public policy.

The behavior of political parties in such coalition bargaining has received substantial scholarly attention. Early applications of cooperative game theory viewed political parties as unconstrained actors seeking to maximize their rewards from executive (cabinet) office through minimal winning coalitions.[1] This first generation of coalition theory assumed that apart from considerations of size (weight), parties were totally indiscriminate in their search for partners (Gamson 1961; Riker 1962). Thus, Conservatives looking for a coalition partner would, for example, be indifferent between Communists and Christian Democrats if both controlled the same number of legislative seats. Due to these radical simplifications, early "policy-blind" coalition theory generated disappointing empirical results. Predictions were often inaccurate and even wildly implausible, and the theory was effectively incapable of explaining either minority or surplus-majority governments, both common phenomena in parliamentary democracies (Laver and Schofield 1990; Lijphart 1984).

Political parties thus frequently defy coalition-theoretic propositions by coalescing too much (surplus-majority coalitions) or too little (minority governments). We shall examine the latter problem—more broadly, the determinants of coalition avoidance—in Norway, a parliamentary democracy where conventional policy-based coalition theory has been found particularly ill fitting (Strøm 1990b). We test institutional as well as policy-based explanations in order to determine whether the Norwegian anomalies might represent structure-induced or preference-induced equilibria (Shepsle 1989). Specifically, we test the explanatory power of one-dimensional policy-based coalition theory. Our analysis identifies party and cabinet policy preferences through content analysis of authoritative party programs and government declarations and systematically examines the effects of institutional rules on bargaining.

The results show that while the institutional environment has indeed favored coalition avoidance, Norwegian parties have not been tightly constrained in their bargaining. Their failure to coalesce results less from inability or institutional restrictions than from the parties' policy preferences. We show that the Norwegian policy space can be plausibly represented by a single, left–right dimension. The bargaining advantages of the core (median) party in such a policy space go a long way toward explaining coalition avoidance. In some cases, however, bargaining constraints or alternative objectives must be invoked to explain coalitional behavior. We use these results to discuss the merits and limitations of policy-based coalition theory generally.

We describe the Norwegian record of coalition avoidance and discuss the treatment of policy and institutions in the coalition-theoretic literature. We then return to the Norwegian case and account for constraints on coalition bargaining. A discussion of coalition bargaining and portfolio allocation sets the stage for a spatial analysis of the contents of party programs and government declarations, documents that help us identify the policy goals of Norwegian parties, as well as their ramifications for cabinet formation. We then present a more formal analysis of the explanatory power of institutional structure, bargaining process, and policy preferences and, finally, summarize our results and reassess the merits of policy-based coalition theory.

NORWEGIAN PARTIES AND COALITION AVOIDANCE

The early development of Norwegian mass parties based on long-lasting social cleavages led to what Lipset and Rokkan (1967) called a "frozen" party system. Only in 1989 was the first independent candidate of this century elected to the Storting (the Norwegian Parliament). No party formed after World War II has been admitted to the cabinet, and few have gained parliamentary representation. Yet the party system has recently experienced several electoral shocks, most notably in 1973 and 1989. Effectively, the postwar history of the party system breaks down into three periods: social democratic predominance, stable two-bloc competition, and polarization and fractionalization.

Social Democratic Predominance (1945–61). Early postwar Norway witnessed a stable five-party system pitting the dominant Labor party (Arbeiderpartiet [A]) against four nonsocialist parties: the Conservatives (Høyre [H]), the Liberals (Venstre [V]), the agrarian Center party (Senterpartiet [SP]),[2] and the Christian People's party (Kristelig Folkeparti [KRF]). After the onset of the Cold War, the Communist party (Norges Kommunistiske Parti [NKP]) gradually faded into oblivion. From World War II until 1961, the Labor party was predominant (Sartori 1976). Four consecutive elections yielded outright parliamentary majorities and single-party Labor governments.

Stable Two-Bloc Competition (1961–73). The 1960s brought a new parliamentary party: the Socialist People's party (Sosialistisk Folkeparti [SF]), whose emergence eroded Labor's support, while the nonsocialist parties gained in strength and cohesion. However, electoral volatility remained low until the early 1970s, and the party system deviated only slightly from its previous five-party format. The socialist and nonsocialist blocs were extremely closely balanced, and minority Labor cabinets alternated with bourgeois (nonsocialist) coalitions.

Polarization and Fractionalization (1973–90). The 1972 European Community (EC) referendum caused a "political earthquake," one manifestation of which was the 1973 formation of the antistatist Progress party (Fremskrittspartiet [FRP]). The Liberals in 1972 acrimoniously split in two, both parts of which have since lost parliamentary representation. An anti–EC fragment of the Labor party exited and joined the Socialist People's party and some former communists in the new Socialist Left party (Sosialistisk Venstreparti [SV]). Long-term results were a substantially weakened Labor party, a temporary Conservative surge, and the eventual atrophy of the nonsocialist center. Party system trends have been toward fragmentation, volatility, and greater competitiveness. Yet the two major parties (Labor and the Conservatives) weathered the crises reasonably well until 1989, when their combined vote share fell from 71.2% to 56.5%.

The Cabinet Record

Postwar cabinets, shown in Table 1, closely reflect this history. Prior to 1961, Norway experienced stable, single-party, majority governments. After that watershed election, however, cabinets have typically been less stable, more often than not "undersized," and only occasionally coalitions. Labor has never again captured a legislative majority, and its cabinets have consequently relied on largely informal and ad hoc *legislative* coalitions. The following patterns of cabinet formation have remained stable:

1. All cabinets have been either socialist or nonsocialist. There have been no peacetime coalitions of socialist and nonsocialist parties. In fact, the Labor party is the only major social democratic party in Western Europe never to have entered a cabinet coalition with any bourgeois party.
2. Labor has eschewed coalitions not only with nonsocialist parties but also with any smaller party to its left. In several campaigns, the Labor party has indeed made a campaign issue out of its resistance to coalition politics. Thus, a socialist government has meant a cabinet of Labor alone—since 1961, therefore, also a minority cabinet.
3. Nonsocialist cabinets, on the other hand, have tended to be coalitions (Rommetvedt 1984); and all but one have included at least three parties. The exception is the first Willoch cabinet (1981–83), a purely Conservative administration.[3] Yet many coalitions, including the two most recent ones, have been minority cabinets.

Coalition politics thus features two-bloc competition within a multiparty system. More precisely, a once-dominant party has alternated in power with a bloc of challengers. The paucity of coalitions, the resulting frequency of minority governments, and the bifurcation of the party system into socialist and nonsocialist blocs are all noteworthy features. Specifically, we notice the *coalition avoidance* in Norwegian party politics. Since 1961, Norway has experienced 14 minority and only 3 majority governments. Seven of these cabinets have been true coalitions, whereas 10 have consisted of only one party. Thus, coalitions have failed to form in most situations where they would conventionally be predicted. Moreover, in 4 of the 7 cases in which (nonsocialist) coalitions *have* formed, coalition building has stopped short of a majority. Even in these cases, then, some coalition "avoidance" has taken place. Hence, the failure to coalesce is general and striking.

TABLE 1

Norwegian Governments 1945–90

GOVERNMENT	TENURE	PARTIES AND SEATS[a]					PARLIAMENTARY SUPPORT[b]
		A	V	SP	KRF	H	
Gerhardsen I	Nov. 45–Oct. 49	*76*	—	—	—	—	50.7 (76/150)
Gerhardsen II	Oct. 49–Nov. 51	*85*	—	—	—	—	56.7 (85/150)
Torp I	Nov. 51–Oct. 53	*85*	—	—	—	—	56.7 (85/150)
Torp II	Oct. 53–Jan. 55	*77*	—	—	—	—	51.3 (77/150)
Gerhardsen III	Jan. 55–Oct. 57	*77*	—	—	—	—	51.3 (77/150)
Gerhardsen IV	Oct. 57–Sept. 61	*78*	—	—	—	—	52.0 (78/150)
Gerhardsen V	Sept. 61–Aug. 63	*74*	—	—	—	—	49.3 (74/150)
Lyng	Aug. 63–Sept. 63		14	16	15	*29*	49.3 (74/150)
Gerhardsen VI	Sept. 63–Oct. 65	*74*	—	—	—	—	49.3 (74/150)
Borten I	Oct. 65–Sept. 69	—	18	*18*	13	31	53.4 (80/150)
Borten II	Sept. 69–Mar. 71	—	13	*20*	14	29	50.6 (76/150)
Bratteli I	Mar. 71–Oct. 72	*74*	—	—	—	—	49.3 (74/150)
Korvald	Oct. 72–Oct. 73	—	5	20	*14*	—	26.0 (39/150)
Bratteli II	Oct. 73–Jan. 76	*62*	—	—	—	—	40.0 (62/155)
Nordli I	Jan. 76–Sept. 77	*62*	—	—	—	—	40.0 (62/155)
Nordli II	Sept. 77–Feb. 81	*76*	—	—	—	—	49.0 (76/155)
Brundtland I	Feb. 81–Oct. 81	*76*	—	—	—	—	49.0 (76/155)
Willoch I	Oct. 81–June 83	—	—	—	—	*53*	34.2 (53/155)
Willoch II	June 83–Sept. 85	—	—	11	15	*53*	51.6 (79/155)
Willoch III	Sept. 85–May 86	—	—	12	16	*50*	49.7 (78/157)
Brundtland II	May 86–Oct. 89	*71*	—	—	—	—	45.2 (71/157)
Syse	Oct. 89–Nov. 90	—	—	11	14	*37*	37.6 (62/165)
Brundtland III	Nov. 90–	*63*	—	—	—	—	38.2 (63/165)

Source: Nordby, 1985; Torp 1990.

Note: A = Det Norske Arbeiderparti (Labor party); H = Høyre (Conservatives); KRF = Kristelig Folkeparti (Christian People's party); SP = Senterpartiet (Center party) which until 1959 was the Bondepartiet (Farmers' party); V = Venstre (Liberals).

[a] Figures for the prime minister's party are in italics.

[b] Figures are percentages of parliamentary seats.

COALITION THEORY, POLICY, AND INSTITUTIONS

There are essentially two ways to explain coalition avoidance. Either the paucity of coalitions may be caused by strong party concerns with policy consistency, or it may be the product of institutional and other constraints on the ability of parties to coalesce. In other words, the frequency of undersized and/or single-party governments may reflect *preference-induced* or *structure-induced* equilibria (Shepsle 1989). Since early coalition theory was "policy-blind," as well as "institution-free," it was unable to capture either of these possibilities. We shall consider how policy concerns and institutions have been incorporated in subsequent scholarship.

Policy-based Coalition Theory

Policy-based coalition theory emerged as a critique of "policy-blind" models (Axelrod 1970; De Swaan 1973; Laver and Schofield 1990; Leiserson 1966). It assumes that "considerations of policy are foremost in the minds of the actors and that the parliamentary game is, in fact, about the determination of major government policy. Coalitions are evaluated according to the proximity of their expected policy to the party's program" (De Swaan 1973, 88). Though this assumption is rarely explicit, policy-based coalition theory allows for greater recognition of the bonds between party leaders and voters (see Laver and Schofield 1990; Luebbert 1986). Presumably, party leaders pursue policy objectives at least in part because of voter demands. Over the course of roughly two decades, policy-based coalition theory has gradually gained the support of most students of multiparty democracy (for recent surveys, see Budge and Laver 1986; Laver and Schofield 1990). Yet conventional policy-based coalition theory rests on theoretical and operational stylizations that are increasingly questioned.

Connectedness and Range. Axelrod's (1970) policy-based coalition theory predicts minimal-connected-winning coalitions. Connectedness requires that coalitions consist of parties that are adjacent in ordinal policy orderings and that all the parties within the coalition's policy range be brought in. There are two important problems with this concept, however. First, connectedness presumes a unidimensional policy space. If parties compete in more than one policy dimension, then connectedness has no well-defined meaning. Second, even in a single dimension, if cardinal policy measures exist, connectedness may be a very poor measure of *compactness*. Consider, for example, a hypothetical one-dimensional policy space bounded by the values 0 and 100, where three equally large parties—A, B, and C—with Euclidean preferences have ideal points 0, 1, and 100, respectively. Coalitions AB and BC would both qualify as minimal-connected-winning; yet the former coalition is far more policy-compact, and should, all else equal, be preferred by party B. Leiserson (1966) and De Swaan (1973) take account of policy compactness in their *minimal-range* theories, in which they predict coalitions that minimize the cardinal policy space between their most distant members. High-quality cardinal policy measures have, however, been difficult to develop, and minimal-range theories remain confined to one-dimensional policy spaces.

Policy Measures. Policy-based (and other) coalition theory has often been tested against crude and impressionistic policy data. To make things worse, such data have been susceptible to contamination. Typically, parties have been ranked according to subjective or "expert" judgments often assumed invariant for long periods, such as the entire post–World War II era. Even more troubling, however, is the distinct possibility that such judgments might be influenced by observed coalition behavior. The Norwegian Center party and the Liberals, for example, might be judged to be close to each other in policy space *because* they have often formed joint coalitions. There is an obvious danger of circular reasoning when these rankings are used in turn to postdict patterns of coalition formation (Lijphart 1984, 55; Luebbert 1983).

Majority Versus Viability. Coalition theorists have only recently begun to clarify the relationship between policy pursuit and other party objectives. Typically, policy concerns have been posited as an objective in addition to power (office), and the relationship between the pursuits of office, votes, and policy has rarely been adequately clarified (see Laver and Budge 1992; Strøm 1990a). Predictions of coalitions that are both spatially compact (policy) and minimal-winning (office) reflect this confusion. Lacking a sufficiently precise and parsimonious specification of party motivations, the literature has often failed to see the full implications of policy assumptions. Recent years have witnessed a third generation of policy-based coalition theory that rejects the size (majority) criterion frequently imported from office-based coalition theory. If parties truly have strictly policy objectives, then all coalitions that cannot be defeated on policy grounds should be feasible. A *policy-viable* coalition is one that cannot be defeated by any other coalition representing a policy position preferred by a legislative majority (see Budge and Laver 1986; Laver and Schofield 1990). Policy viability corresponds to the game-theoretic concept of the core of a voting game. In a unidimensional policy space, the core consists of the party controlling the median legislator. If parties are purely policy-motivated, then no majority exists that could rationally agree to shift the policy position away from that of the median party. Consequently, the core party should be able to prevail even where it controls far less than a parliamentary majority. This provides a simple and powerful explanation of minority cabinets including the core party, with or without additional participants (Laver and Schofield 1990; Strøm 1990b).[4]

Policy Space and Dimensionality. While policy viability represents a simple and elegant theory of coalition formation, it retains a number of strong assumptions. Like most applications of coalition theory to multiparty executives, it generally presumes a unidimensional policy space. This, of course, is a heroic assumption, since we know from behavioral research that many political systems exhibit multiple dimensions of electoral competition. Yet unidimensionality is essential for any analytical results, since it is generally impossible to guarantee stable solutions in higher dimensionalities. In technical terms, the set of majority-undominated solutions (the core) is generically empty in multidimensional space, giving rise to endless cycles whereby contending coalitions can defeat one another (McKelvey 1976).

Institutional Explanations

Since their inception, coalition-theoretic studies of cabinet formation have developed more adequate accounts of party objectives and their consequences. The institutional environment in which parties bargain has been less successfully incorporated in the theoretical literature (Strøm, Budge, and Laver n.d.). Policy-based coalition theory has generally failed to incorporate the effects of institutional rules and structures. Its equilibria have been entirely preference-induced, as opposed to structure-induced (Shepsle 1989), and this has no doubt contributed to its modest explanatory power. In reality, of course, cabinet-level politics is played out in heavily institutionalized environments, and parties commonly go into bargaining situations under a variety of constraints and precommitments.[5]

Coalition theory has, for example, long ignored cabinet decision-making processes. Policy-based coalition theory implicitly assumes that bargaining parties can determine and enforce the coalition's policy position at will throughout the relevant policy space. There are two theoretically important institutional challenges to this assumption. Laver and Shepsle (1990) propose a model wherein individual cabinet ministers are dictators within their policy jurisdictions. Under these conditions, the set of feasible coalition policy positions is sharply reduced, which may account for coalition avoidance. Such an institutional environment would also throw into doubt any theory premised on unidimensionality. An alternative conception popularized in the British television comedy series *Yes, Minister* is that bureaucrats ultimately decide policy much as they like, so that coalition policy agreements cannot be enforced. To the extent that this is true, only the more cynical postulates of office-based (or "policy-blind") coalition theory remain plausible: party leaders seek office because of the intrinsic value of its spoils.

Policy-based (and other) coalition theory typically also ignores the institutional bargaining environment and the constraints imposed by the rules of government formation. Models based on cooperative coalition theory cannot adequately account for the effects of such rules as the sequence in which parties are allowed to make or accept bids for cabinet participation. In seeking to characterize very general solutions, cooperative coalition theory essentially abstracts away from such "arbitrary" processes. This lack of attention to the bargaining process is a theoretical liability. In his seminal work, Riker (1962) explicitly describes coalition formation as a progressive expansion of protocoalitions. Yet cooperative coalition theory has never found a way to incorporate this understanding of the bargaining process into its theoretical fabric. In most works on cabinet coalitions, bargaining is implicitly assumed to be completely unconstrained, such that parties are free to make or break agreements at will.[6] In the real world, bargaining constraints exist and may be responsible for coalition avoidance.

INSTITUTIONS AND BARGAINING CONSTRAINTS IN NORWAY

We have discussed the two potential explanations of anomalous coalitional behavior, namely, policy concerns and institutional constraints. Our general purpose here is to develop and test these explanations in the Norwegian case. Specifically, we shall explore the potential and limits of a policy-based explanation of coalition avoidance. However, we shall first examine Norwegian institutional constraints and party precommitments in coalition bargaining. Our objective is to examine both the independent effect of such factors on the set of feasible coalitions and the extent to which their recognition can enhance the explanatory power of policy-based coalition theory.

A constraint on coalition bargaining is any restriction on the set of feasible coalitions that is beyond the short-term control of the players (Strøm, Budge, and Laver n.d.). Not all constraints are equal. We distinguish between hard constraints, which effectively eliminate particular combinations of parties, and soft constraints, which simply make particular coalitions more or less likely. Hard constraints must be both specific and enforceable; that is, they must clearly specify which coalitions are or are not permissible, and they must be backed up by credible enforcement mechanisms. Soft constraints, which are less specific and/or less enforceable, simply reflect how institutional arrangements "load the dice" in coalition bargaining. In large part, constraints are *institutions*, by which we mean "rules about behavior, especially about making decisions" (Riker 1980, 432). Although our interest is in the formation of executive coalitions, the relevant institutional environment is not narrowly limited to the cabinet formation process. Coalition bargaining may be affected by rules operating in various other arenas where parties interact. Thus, the relevant constraints include rules concerning (1) cabinet formation, (2) cabinet decision making, (3) legislative procedure, and (4) legislative elections. Party precommitments, essentially intraparty rules, are a particular form of constraint. Empirically, any bargaining constraint must be identifiable ex ante, so that we may avoid the descent into post hoc hypothesis saving.

Cabinet Formation Rules

There are few constitutional restrictions on Norwegian cabinet formation. The 1814 Constitution has never been amended to reflect the introduction of parliamentary government in 1884 and consequently contains few relevant restrictions concerning government formation. Though the king formally selects his council at will, he has in practice exerted no personal influence since 1928 and normally follows the outgoing prime minister's advice (Björnberg 1939). In especially difficult situations, the monarch may consult the parliamentary party leaders or the president of the Storting. The latter, however, has played a prominent role in cabinet bargaining only in 1971, after Per Borten's four-party nonsocialist coalition disintegrated.[7] Since parliamentary government has never been constitutionally recognized, there is no formal vote of investiture. This constitutional permissiveness has facilitated the swift resolution of most cabinet crises. Typically, negotiations have been completed within a few days, and the initial *formateur* has succeeded (Strøm 1990b).

Cabinet and Legislative Decision Rules

The procedures by which Norwegian cabinets and legislatures operate only indirectly constrain coalition bargaining. Cabinet decision rules have, since the nineteenth century, evolved to reflect parliamentary government. These procedures are not highly codified, though rules and conventions exist in the constitution and in the cabinet's standing orders. Collective cabinet responsibility to the legislature is recognized, and the need for coalition partners to share responsibility for cabinet policy may promote policy-compact coalitions, since party leaders eschew coalitions in which they might have to defend compromises that are unpalatable to their followers. Parliamentary procedure is similarly informal and permissive (Rasch 1987). Incumbent governments need only submit to a vote of confidence at their own volition or the opposition's insistence. Both are rare events. Consequently, a government's parliamentary support may be ambiguous (Stavang 1971), which facilitates minority governments that are tolerated, if not supported, by opposition parties. Incumbents need not resign at election time or when a new head of state accedes to the throne, which favors continuity in government. The Constitution requires parliamentary supermajorities for constitutional amendments (e.g., reforms of the electoral system) and for membership in supranational organizations entailing concessions of sovereignty (e.g., the EC). Yet the need for qualified majorities has never seriously affected cabinet bargaining.

Electoral Rules

The electoral system is an especially important constraint on coalition bargaining. Norway has a proportional representation system with district magnitude ranging from 4 to 15. Until 1989 there were no supplementary seats. Due to small districts and the lack of supplementary seats, legislative disproportionality has consistently been far higher than elsewhere in Scandinavia and among the highest of all proportional representation countries (Särlvik 1983; Strøm 1985). This has given the nonsocialist parties (and particularly the smaller ones) strong incentives to form electoral coalitions in the form of joint lists, which have been very frequent. In two elections (1945 and 1985) the even more attractive option of *apparentement* has been available. These forms of preelectoral coalition building have clearly facilitated nonsocialist *cabinet* coalitions as well. An unusual constitutional feature is the lack of any provision for early parliamentary dissolution. Norway is the only Western European state without this "safety valve." This constitutional anomaly affects coalition bargaining in various ways. The impossibility of early elections reduces uncertainty concerning cabinet durability. It eliminates one incumbency advantage (the authority to schedule parliamentary elections) and probably also induces toleration of numerically weak cabinets in difficult parliamentary circumstances. Thus its main effect as a soft constraint may be to facilitate minority governments.

Party Precommitments

Norwegian parties have frequently precommitted themselves in ways that have had a direct bearing on coalition bargaining. Such constraints have been particularly significant under nonsocialist governments. Both times that such coalitions have faced elections (1969 and 1985), they have pledged continued cooperation in the event that they retained their legislative majorities. Similarly, in 1977 the three major nonsocialist parties (the Conservatives, the Center party, and the Christian People's party) precommitted themselves to a cabinet coalition in the event of a parliamentary majority. Occasionally, a subset of two or three bourgeois parties have similarly precommitted themselves as in 1983, when the Center party made its coalition with the Conservatives conditional on the cabinet participation of the Christian People's party. In 1989 the same three parties made the less committal agreement to negotiate in the event of a nonsocialist majority. As the nonsocialists did win, negotiations duly followed, leading to the Syse government. But since a successful bargaining outcome was by no means a foregone conclusion, we have not counted this joint declaration as a formal constraint.

The Labor party is an important and interesting case of a *proscriptive* party precommitment, one that rules out specific coalitions. In August 1972, Prime Minister Trygve Bratteli issued his famous "confidence motion" on EC membership, threatening to resign if the referendum the following month failed to approve Norwegian entry. When the majority of the voters nonetheless rejected membership, Bratteli effectively had no choice but to resign and keep his party out of the subsequent coalition bargaining. Any other course of action would most likely have been severely penalized in the parliamentary election less than a year later. Labor's refusal to consider coalition participation in 1972, however, is only the most explicit case of a long-standing practice. In the 1930s, the party decided to eschew cabinet coalitions, an electorally very successful choice that has become a Labor tradition: the party has never even entered coalition bargaining. Yet it is not clear that this tradition has been binding on party behavior. In fact, the possibility of entering a cabinet coalition has occasionally been raised, most notably in 1961 and 1973.

These bargaining constraints are summarized in Table 2. The effective hard (specific and enforceable) constraints, all of which are all party precommitments (rows 6, 7, and 9), collectively eliminate 126 potential solutions across four bargaining situations. This amounts to fewer than 8% of all potential coalitions since 1961. The softer constraints are consistent with the observed pattern of cabinet formation. They generally facilitate minority cabinet formation, while several constraints (e.g., rows 3, 4, and 10) also foster nonsocialist cooperation. In sum, coalition politics is affected by a variety of soft—but few hard—constraints. Institutional factors thus help explain the frequency of minority cabinets but at the same time suggest that broader nonsocialist coalitions should have been feasible.

PORTFOLIO BARGAINING

Policy-based coalition theory also depends on implicit or explicit assumptions about the institutional context of policy making. For a one-dimensional policy-based coalition theory to be plausible, coalition decision making must be relatively centralized, aggregative, and enforceable. We would expect policy negotiations to be a key part of coalition bargaining, and we must be able to reject models wherein parties abdicate policymaking either to civil servants or to individual cabinet ministers with clearly disaggregated competencies along distinct policy dimensions.

There have been only nine postwar cases of coalition bargaining, of which five (1963, 1965, 1972, 1983, and 1989) have led to coalition cabinets. In three cases (1971, 1981, and 1990), no coalition agreement was reached, and single-party minority cabinets formed instead. In 1987, the three major nonsocialist parties reached agreement but failed to dislodge Brundtland II due to the lack of support from the Progress party (Strøm n.d.). Judging from authoritative accounts

TABLE 2

Constraints on Coalition Bargaining in Norway

TYPE	SUBSTANCE	OPERATIVE PERIOD	EFFECT
Cabinet formation rule			
	1. No need for government to resign at election time	All years	Favors status quo government
Cabinet operation rule			
	2. Collective cabinet responsbility	All years	Favors policy-compact government
Electoral rules			
	3. *Apparentement*	1945, 1985	Predisposed nonsocialist parties toward coalition
	4. Low district magnitude causing disproportionality	All years	Predisposes smaller nonsocialist parties toward coalition
	5. No provision for early disolution of Parliament	All years	Decreases uncertainty in coalition bargaining; favors minority governments
Party precommitments			
	6. If majority retained, continuation of Borten government	1969	Dictated government solution
	7. If EC membership rejected, Labor party withdrawal from government	1972	Necessitated nonsocialist government
	8. If three-party nonsocialist majority (H, KRF, SP), coalition of these parties	1977	No such majority, but it disfavored coalition of any of these parties with Labor
	9. Center party coalition with Conservatives contingent upon participation of Christian People's party	1981, 1983	Reduced set of nonsocialist solutions
	10. If majority retained, continuation of Willoch government	1985	Favored this coalition even without majority

(e.g., memoirs), policy compromise indeed appears to have been the critical factor in most coalition negotiations, although portfolio allocation has also often been vigorously contested, for example in 1965 (Lyng 1976). When bargaining has broken down, as it did in 1971, 1981, and 1990, it has been over policy issues (EC membership in 1971 and 1990, abortion in 1981). Moreover, in focusing on formal coalition bargaining we may underestimate policy concerns, since the relevant parties frequently reach policy compromises in legislative bargaining even before cabinet negotiations begin.[8]

Theoretical Expectations

Portfolio allocations contain important information that allows us indirectly to assess the plausibility of a unidimensional policy theory by examining the predictions of the two competing institutional models (ministerial vs. bureaucratic control). Each of these alternative institutional assumptions generates fairly straightforward predictions concerning portfolio allocation. First, to the extent that cabinet ministers make policy segmentally and autonomously, as argued by Laver and Shepsle (1990, 1993), we should see strong regularities in the distribution of ministerial appointments across the coalition parties. Under these conditions and absent a "strong" party, ministerial portfolios should go to the median party on each dimension. Even when one party is "strong," there is a "strong centripetal tendency" (idem 1993, 28), and portfolio allocation should not deviate sharply from the dimension-by-dimension median. Important features of Norwegian policymaking lend credence to this model. Cabinets typically include all heads of departments (ministries), and only rarely are ministers without portfolio appointed. Studies of national policymaking often stress the segmentation of this process (Olsen 1983). The latter characterization, however, is also consistent with the alternative model, that of bureaucratic control. If bureaucrats ruled and parties valued office mainly for its intrinsic rewards, we should see more concern with each party's numerical share of portfolios, whereas the particular offices should be allocated in a more random fashion, determined mainly by the prestige, perquisites, and patronage powers of the various offices.

Portfolio Allocation

Numerically, portfolio allocations in nonsocialist coalitions generally correspond closely to the parity norm (Browne and Franklin 1973; Gamson 1961). In recent years, however, the smaller centrist parties have been somewhat overrepresented (see Table 3).[9] While these results are compatible with bureaucratic control, a closer look at portfolio allocation belies this notion. There is a striking association between the traditional policy concerns of the various parties and the ministries they have controlled. In all six coalitions in which they have participated, the Conservatives have held the departments of Defense and Industry. In all but one case, the party has controlled the Justice Department as well, and in four coalitions

TABLE 3

Portfolio Allocation in Norwegian Coalition Cabinets, 1963–90

PORTFOLIO[a]	LYNG 1963	BORTEN I 1965–69	BORTEN II 1969–71	KORVALD 1972–73	WILLOCH II 1983–85	WILLOCH III 1985–86	SYSE 1989–90
Prime minister	H	SP	SP	KRF	H	H	H
Agriculture	SP	V	V	SP	SP	SP	SP
Church and education	V	KRF	KRF	SP	KRF	KRF	KRF
Consumer affairs	—	—	—	V	H	H	—
Cultural affairs	—	—	—	—	H	H	KRF
Defense	H	H	H	V	H	H	H
Environment	—	—	—	—	SP	SP	SP
Family affairs	SP	KRF	KRF	—	—	—	KRF
Finance	SP	V	V	V	H	H	H
Fisheries	H	SP	SP	SP	H	SP	H
Foreign affairs	KRF	H	H	SP	H	H	KRF
Foreign aid	—	—	—	—	KRF	KRF	SP
Industry	H	H	H	V	H	H	H
Justice	KRF	H	H	KRF	H	H	H
Labor and administration	—	—	—	—	—	—	H
Municipal affairs	V	V	V	KRF	H	H	SP
Oil and energy	—	—	—	—	KRF	KRF	SP
Social affairs	KRF	KRF	KRF	KRF	H	H	H
Trade	H	H	H	V	KRF	KRF	H
Transportation	SP	H	H	SP	SP	SP	KRF
Wages and prices	V	SP	SP	—	—	—	—
Party shares[b]							
V	.20 (.19)	.20 (.23)	.20 (.17)	.33 (.13)	—	—	—
SP	.27 (.22)	*.20 (.23)*	*.20 (.27)*	.40 (.51)	.17 (.14)	.22 (.15)	.26 (.18)
KRF	.20 (.20)	.20 (.16)	.20 (.18)	*.27 (.36)*	.22 (.19)	.22 (.21)	.26 (.23)
H	*.33 (.39)*	.40 (.39)	.40 (.38)	—	*.61 (.67)*	*.56 (.64)*	*.48 (.60)*

Sources: Nordby 1985; Torp 1990.

Note: H = Høyre (Conservatives); KRF = Kristelig Folkeparti (Christian People's party); SP = Senterpartiet (Center party), which until 1959 was the Bondepartiet (Farmers' party); V = Venstre (Liberals).

[a]Some changes have taken place in the names and jurisdictions of ministries.

[b]Entries are proportions of cabinet portfolios, with proportions of coalition legislative seats in parentheses. Italicized figures represent party of prime minister.

the Conservatives have captured Foreign Affairs. The Center party and the Christian People's party have each participated in seven coalitions, all but one of which have included the much larger Conservatives. Despite being a junior partner, the Christian People's party has captured the important portfolio of Social Affairs on four occasions. Even more impressively, the party has won Church and Education five times out of seven.[10] The Center party has demonstrated a most plausible penchant for Agriculture, Fisheries, and Transportation. It has controlled Agriculture in five coalitions and Fisheries and Transportation four times each. Finally, the Liberals have held Finance and Municipal Affairs three times each in their four coalitions.

While a full analysis of these allocations is beyond our scope here, even a cursory reading strongly suggests that both hypotheses (bureaucratic and ministerial control) must be rejected. Clearly, portfolio allocation is not random, nor do the preferences of the various parties seem mainly to be driven by the prestige or patronage power of the various offices. This assessment is supported by Laver and Hunt's (1992) expert survey. On the question whether cabinet portfolios are valued more as rewards of office or as means to affect policy, Norway ranked higher on policy value than any other country in this 25-nation survey (p. 71). Hence, the attractions of cabinet office cannot plausibly lie in spoils alone. Consider, for example, the Center party under Willoch II, when the party was in the policy core by holding the median legislator. Yet the offices the party gained (Agriculture, Environment, and Transportation) were all ranked quite low in importance by Laver and Hunt's experts (p. 276). Out of the 14 portfolios ranked by these experts, with the prime ministership excluded, the *most* prestigious of the Center party's ministries, Transportation, ranked only ninth. Yet all three portfolios are of great concern to the Center party's main electoral constituencies, namely, the primary sector and the rural population.

The results are similarly unsupportive of Laver and Shepsle's proposition that portfolios should tend go to the median party on each policy dimension. On the contrary, portfolios seem more likely to be allocated to the party for which the policy area is of the greatest salience and that often also holds the most extreme position within the coalition (e.g., the Christian People's party on education, the Center party on agriculture, the Conservatives on defense and industry). Competition over portfolio allocation has often been between parties with extreme preferences. In 1965, for example, the most serious confrontation took place between the Liberals (the most secular coalition party) and the Christian People's party (the most clerical) over the department of Church and Education, which the latter party eventually won (Rommetvedt 1984, 28). The more moderate parties on this dimension, the Center party and the Conservatives, appear not to have harbored any serious misgivings about giving this important department to an obvious "preference outlier" on issues of religion and education (Lyng 1978, 30–31; Strøm and Leipart 1989, 280). Indeed, the Christian People's party has been able to retain this portfolio (or its equivalents) in four out of five subsequent coalitions. The most plausible explanation of this acquiescence by the Center party (the party of the prime minister) and the Conservatives (the largest coalition party) is that they believed they would be able to enforce a comprehensive coalition policy agreement.

The evidence from coalition bargaining and portfolio allocation lends credence to our portrayal of Norwegian parties as policy seekers. We also infer that cabinet decision making is relatively centralized and that coalition parties are able to make aggregated and enforceable policy agreements. These institutional features enhance the plausibility of a policy-based coalition theory.

POLICY POSITIONS

Parties pursue policy objectives at least in part because of the constraints of electoral competition. Accordingly, we look for evidence of policy goals in the "contracts" between party leaders and their followers. We shall content-analyze the most official such contracts, namely, party platforms ("manifestos" in British parlance) and government declarations. Such data are both more authoritative and better suited to the underlying theoretical concerns than are expert judgments or impressionistic rankings. Besides, we avoid the dangers of tautological reasoning inherent in the latter forms of evidence.

Policy Documents

Norwegian parties and governments issue various policy statements. We focus on the most authoritative such documents: election programs (*handlingsprogram*) for all parliamentary parties and all elections from 1949 through 1989 and government declarations for all available postwar governments. We have discussed the party programs elsewhere (Strøm and Leipart 1989), and all documents are described in detail in the Appendix. These documents provide the best available data on party policy positions because they are recognized as authoritative and provide data that are not likely to be contaminated by the very behavior we seek to explain. There is by now a substantial literature employing content analysis of party programs for our purposes (Budge and Farlie 1977; Budge, Robertson, and Hearl 1987; Laver and Budge 1992; Robertson 1976). Theoretically and methodologically, we build on this literature. We have content-analyzed both party programs and government declarations using a common 55-category coding scheme, consisting of seven main domains (see the Appendix; see also Laver and Budge 1992; Strøm and Leipart 1989). Each sentence, or quasi sentence, in each party or government document, has been assigned to one and only one coding category. In accordance with saliency theory, we measure the salience of each theme by its relative frequency (Budge, Robertson, and Hearl 1987).

Policy Dimensions

The policy dimensions of Norwegian electoral competition closely correspond to social cleavages in the party system (Rokkan 1970). In analyses of party programs, the left–right dimension, corresponding to the class cleavage, has consistently emerged as the most significant (Bilstad 1986; Grønmo 1975a, 1975b; Pettersen 1973, 1979; Valen and Grønmo 1972).[11] Our factor-analysis of party programs between 1949 and 1985, using a slight modification of the coding scheme developed by Budge, Robertson, and Hearl (1987), found that the left–right dimension predictably emerged as the most salient (Strøm and Leipart 1989). Out of the four factors extracted and interpreted, two were related to the left–right socioeconomic policy dimension. Other studies employing various data and methods confirm that the left–right dimension is the dominant ordering of the party system (Knutsen 1989; Listhaug, Macdonald, and Rabinowitz 1990; Shaffer 1991).

The Left–Right Scale

As noted, policy-based coalition theory has far greater explanatory value if party preferences can be plausibly represented in a unidimensional policy space. Moreover, it is under such assumptions, where a policy core is guaranteed to exist, that policy viability adds particular analytical power. In the interest of theoretical parsimony and given the demonstrated centrality of the left–right dimension, we therefore impose a unidimensional left–right scale on our analysis. While the available evidence suggests that this is a plausible representation of the Norwegian party system, the ultimate proof of the pudding is in the analytical results it generates. This left–right dimension was developed in a larger cross-national study so as to include all items with a strong a priori presumptive left–right content. These associations were subsequently tested against broadly cross-national data. Only coding categories that had face validity as components of a left–right dimension and actually clustered in this way cross-nationally were retained in the final measure (Laver and Budge 1992).

TABLE 4

Policy Positions of Parties and Governments

ELECTORAL PERIOD	PARTIES							GOVERNMENT		
	NKP/SF/SV	A	V	SP	KRF	H	FRP	1	2	3
1945	—	—	—	—	—	—	—	−7.3	—	—
1949	−36.4	*−37.2*	−.9	16.7	20.9	18.9	—	−25.9	9.1	—
1953	−18.0	*−31.2*	−22.1	13.4	19.1	18.0	—	−20.8	−17.4	—
1957	−44.9	*−32.7*	−1.6	12.2	10.5	13.2	—	−33.3	—	—
1961	−41.0	*−27.4*	−18.3	−5.8	8.2	5.6	—	−31.8	−2.4	—
1965	−49.5	−40.0	*−34.3*	−15.1	−3.4	−4.3	—	−16.2	—	—
1969	−49.2	−35.1	*−31.2*	−17.8	−19.0	−1.4	—	−24.2	−34.0	−21.2
1973	−42.0	*−36.9*	−30.7	−14.4	−5.0	−17.9	70.2	−45.1	−8.3	—
1977	−37.3	*−32.1*	−22.3	−19.0	−5.5	−10.1	22.1	−20.9	−17.1	—
1981	−39.7	−23.1	−22.6	*−14.9*	−7.9	8.1	32.4	−3.3	1.1	—
1985	−36.3	−25.9	−22.0	*−12.9*	−12.0	−8.0	34.4	−14.0	−6.1	—
1989	−35.1	−35.9	−20.5	−19.7	*−21.4*	−7.0	24.1	9.1	—	—

Note: Governemnts counted chronologically. High scores indicate right-wing positions. Italicized entries represent the party controlling the median legislator. FRP = Fremskrittspartiet (Progress party); NKP/SF/SV = Norges Kommunistiske Parti (Communist party), 1949–57; Sosialistisk Folkeparti (Socialist People's party), 1961–69; Sosialistisk Valgallianse (Socialist Electoral Alliance), 1973; and Sosialistisk Venstreparti (Socialist Left party), 1977–89. A = Det Norske Arbeiderparti (Labor party); H = Høyre (Conservatives); KRF = Kristelig Folkeparti (Christian People's party); SP = Senterpartiet (Center party), which until 1959 was the Bondepartiet (Farmers' party); V = Venstre (Liberals).

Scores were calculated by adding up the relative frequencies (in percentage terms) of all rightist statements and subtracting from that the total frequency of leftist themes. Scores thus vary between hypothetical bounds of −100 and 100, with high scores reflecting a preponderance of rightist policy statements (see Appendix).[12] We use this left–right scale to examine four aspects of coalition politics: (1) the policy positions taken by the various parties in their electoral platforms, (2) the governments' positions in the same policy space, (3) coalition formation, and (4) policy payoffs to parties inside and outside the cabinet. If parties are truly policy seekers and the left–right scale indeed represents the most important dimension of interparty competition, cabinets should form between parties that are close to each other in this policy space. We also expect these cabinets to take policy positions close to the ideal points of the participating parties.

Party Policy Positions

As Table 4 and Figure 1 show, Norwegian parties exhibit fairly stable and intuitively plausible left–right positions, which are strikingly consistent with conventional placements based on surveys and other evidence.[13] Individual party positions are consistent with conventional placements in 180 out of 195 pairwise comparisons (92.3%). Most deviations (10 out of 15) involve the Christian People's party, which in six elections, contrary to conventional wisdom, is to the right of the Conservatives.[14] No other party consistently occupies a counterintuitive policy position. Most deviations from conventional placement occur in the early postwar years. The greater consistency in recent elections may reflect the fact that party programs have become more elaborate and standardized and that parties devote greater resources to their preparation.[15]

Government Policy Positions

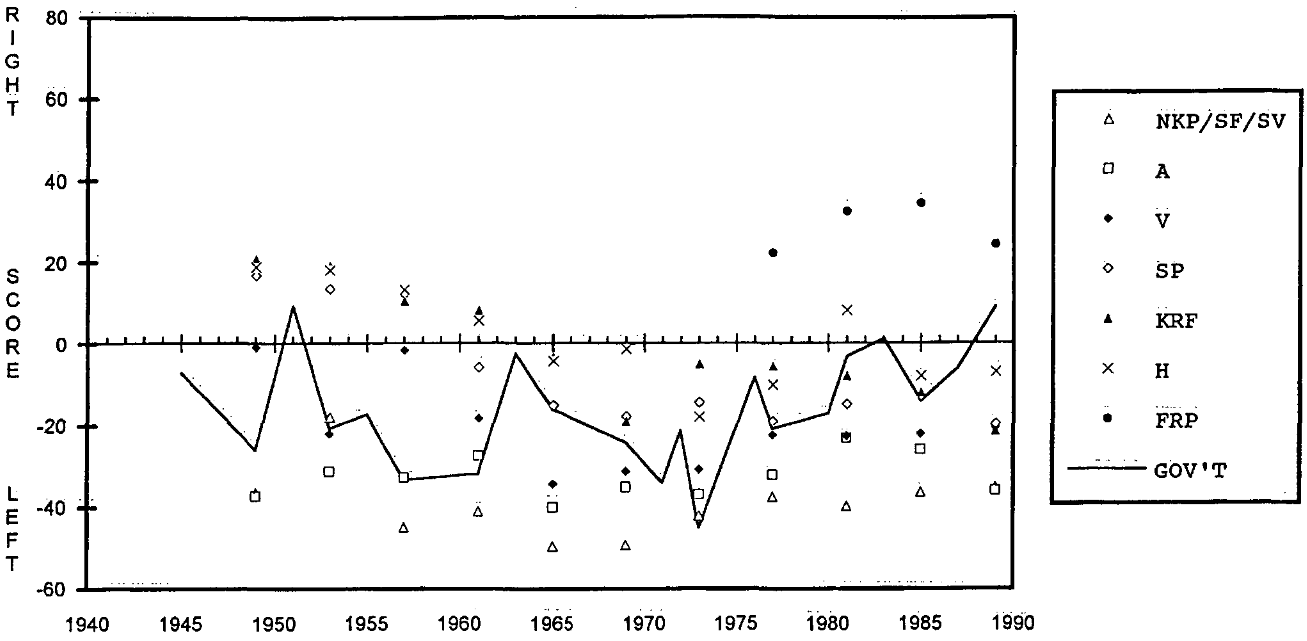
Most cabinets have taken moderate positions in left–right space, a result consistent with conventional wisdom. The most deviant case is 1973, when Bratteli presented the most leftist postwar government declaration—to the left even of the Socialist Left. While the prime minister personally represented the moderate wing of the Labor party, he was in 1973 severely challenged by the Socialist Left as well as by Labor's left wing, which had already experienced significant exit. His leftist program may therefore have been a defensive move. Early postwar Labor cabinets, on the other hand, were unusually conservative, most notably Torp I (see Appendix). Policy shifts from one government to the next are quite plausible. Transitions between socialist and nonsocialist cabinets are in all but one case accompanied by substantial policy shifts in the expected direction. The movements in government policy positions in 1963, 1965, 1971–73, 1981, and 1989 are thus consistent with expectations. The "inconsistent" case is Brundtland II, which took a position to the right of its bourgeois predecessor. Her apparent conservatism may in part reflect the government's austerity policy in response to the collapse of world oil prices.

Government Formation

Our data allow us to examine several theoretically interesting coalition properties, including policy range, connectedness, and the explanatory power of core theory. Table 5 shows how Norwegian cabinets have conformed to these expectations and reviews

FIGURE 1

Party and Government Policy Positions



the time-specific constraints (Table 2, rows 3, 6–10) in each bargaining situation. The governments up to and including Gerhardsen IV are of limited theoretical interest, because a majority Labor cabinet is so obviously the dominant solution. All pre-1961 cabinets have therefore been excluded from this analysis. Note that every single cabinet has been connected. This is trivially true for single-party governments, but it is a noteworthy feature of every coalition as well. Policy consistency, then, provides a valuable key to cabinet formation. It is more difficult to interpret the record of policy range.[16] Clearly, the internal policy distance between governing parties has tended to decline over time, but this may be due less to any growing preference for compact coalitions than to the fact that, except for the Progress party, left–right

TABLE 5

Characteristics of Norwegian Governments, 1961–90

GOVERNMENT	SPECIFIC CONSTRAINTS[a]	SIZE	POLICY-VIABLE[b]	CONNECTED	POLICY RANGE[c]
Gerhardsen V	None	Undersized	Yes	Yes	0
Lyng	None	Undersized	No	Yes	26.5
Gerhardsen VI	None	Undersized	Yes	Yes	0
Borten I	None	Minimal-winning	Yes	Yes	30.9
Borten II	6	Minimal-winning	Yes	Yes	29.8
Bratteli I	None	Undersized	No	Yes	0
Korvald	7	Undersized	Yes	Yes	13.4
Bratteli II	None	Undersized	Yes	Yes	0
Nordli I	None	Undersized	Yes	Yes	0
Nordli II	8	Undersized	Yes	Yes	0
Brundtland I	None	Undersized	Yes	Yes	0
Willoch I	9	Undersized	No	Yes	0
Willoch II	9	Minimal-winning	Yes	Yes	23.0
Willoch III	3, 10	Undersized	Yes	Yes	4.9
Brundtland II	None	Undersized	No	Yes	0
Syse	None	Undersized	Yes	Yes	14.4
Brundtland III	None	Undersized	No	Yes	0

[a] Entries refer to specific constraints enumerated in Table 2.
[b] A policy-viable government is one that includes the median (core) party.
[c] The maximum distance between any pair of parties in the government.

interparty distances have declined since the 1950s. The most interesting results in Table 5, however, concern policy viability. Whereas 14 out of 17 cabinets have failed to meet the size criterion, only five have not been policy-viable. In other words, the majority of the cabinets that have fallen short of majorities have included the core (median) party.

Nonviable Governments

The five nonviable governments (those that have not included the median party) deserve attention mainly because they are so few. Had the one-dimensional policy model not worked so well, these deviant cases would have been less interesting. In order to examine alternative explanations of these cases, we focus on four questions: (1) Were these cabinets close to viability in our policy space? (2) Is there evidence that alternative policy dimensions were consequential? (3) Do they reflect the importance of nonpolicy objectives? and (4) have these cabinets resulted from institutional or other constraints on coalition bargaining?

The nonviable Labor party cabinets for which we have data (Bratteli I and Brundtland II) have positioned themselves close to the core party (in the former case the Liberals and in the latter the Center party). By taking policy positions close to the median party, these Labor prime ministers may have facilitated their survival. Very clearly, however, several nonviable cabinets reflect the intrusion of issues cross-cutting the left–right dimension. For Bratteli I and Brundtland III, this refers to the EC membership issue, which pits the nonsocialist parties against one another. In the early 1970s as well as the late 1980s, the Conservatives were the most committed proponents of EC membership, whereas the Center party was staunchly opposed. In 1981, on the other hand, the divisive issue was abortion, on which most Conservatives were at odds with the prolife policies of the Christian People's party. Although two years later the Conservatives, the Center party, and the Christian People's party managed to form a cabinet coalition, in 1981 their bargaining foundered on this issue, and the Conservatives formed a nonviable single-party cabinet (Willoch I).

John Lyng's short-lived 1963 coalition best illustrates the significance of nonpolicy objectives. Lyng's government declaration represented the policy range of the four coalition parties about as well as possible, but it did nothing to accommodate the core Labor party. Indeed, shortly after taking office, Lyng effectively sealed his fate by confrontationally demanding a vote of confidence, a vote he knew he would lose (Strøm 1990b, 222–23). His principal intention was to signal to the voters the bourgeois parties' ability and willingness to provide an alternative to Labor rule. Lyng's desire for longer-term electoral gains appears to have overshadowed his interest in achieving immediate policy rewards.

Hard bargaining constraints have played only a minor role in fostering nonviable cabinets. In 1981, the Christian People's party's decision not to coalesce with the Conservatives effectively also kept the core Center party out, since the latter had precommitted itself to joining the Conservatives only within a larger coalition. Softer institutional constraints have much more frequently been a factor. Thus, four out of five nonviable cabinets have formed after the failure of a previous cabinet, including the core party. In fact, all the nonviable Labor party administrations have been born in midterm upon the demise of coalitions representing the bourgeois majority. If such a constitutional recourse had existed, these situations might well have occasioned early elections.

Party Payoffs

Party policy payoffs are measured in Table 6 as the policy distance (in left–right terms) between the party in question and the government. The smaller this distance, the greater the policy payoff. While only parties represented in the executive coalition receive office payoffs, even opposition parties may share in policy payoffs. Occasionally, opposition parties (especially those close to the policy core) may even gain policy closer to their ideal points than those in the cabinet. However, Norwegian cabinets have, on average, placed themselves much closer to the governing parties than to the opposition. In fact, the mean distance between governments and opposition parties is twice as large as between governments and governing parties. Labor, however, has enjoyed lesser policy gains from being in office than have most of the bourgeois parties. Many single-party Labor governments have deviated sharply from the party position. While some such discrepancies can be explained by prime-ministerial preferences or by the desire to accommodate the opposition, the magnitudes are often uncomfortably large. The distortionary brevity of some Labor government declarations, as discussed in the Appendix, is a possible explanation. The Liberals (the most leftist nonsocialist party and often close to Labor in policy space) have reaped minimal policy benefits from participation in bourgeois cabinets. Like the Labor party, the Liberals have often been at, or close to, the policy core. Their small relative gains from being in office thus reflect the policy advantages of a core party even in opposition. The other nonsocialist parties have done substantially better in government than in opposition.

Nonsocialist coalitions best approximate the governing parties' policy positions. Lyng and Borten I, for example, took positions close to the mean of their respective coalitions, and Borten II and Willoch II were well within their respective policy ranges. In the 1965, 1972, 1983, and 1989 coalitions, the coalition policy position ended up closest to the prime minister's party. Coalition government declarations thus conform better to expectations than those delivered by single-party cabinets, probably because the former are designed to be more authoritative policy statements. Whereas Labor's cabinets rely on the party manifesto as their principal policy statement, bour-

TABLE 6

Policy Distance Payoffs

	PARTY							MEAN ABSOLUTE DISTANCE (ALL PARTIES)
GOVERNMENT	NKP/SF/SV	A	V	SP	KRF	H	FRP	
Gerhardsen II	11	*11*	−25	−43	−47	−45	—	30
Torp I	46	*46*	10	−8	−12	−10	—	22
Torp II	−3	*10*	1	−34	−40	−39	—	21
Gerhardsen III	1	*14*	5	−31	−37	−35	—	21
Gerharden IV	12	*−1*	−32	−46	−44	−47	—	30
Gerhardsen V	9	*−4*	−14	−26	−40	−37	—	22
Lyng	39	25	*16*	*3*	*−11*	*−8*	—	17
Borten I	33	24	*18*	*−1*	*−13*	*−12*	—	17
Borten II	25	11	*7*	*−6*	*−5*	*−23*	—	13
Bratteli I	15	*1*	−3	−16	−15	−33	—	14
Korvald	28	14	*10*	*−3*	*−2*	−20	—	13
Bratteli II	−3	*−8*	−14	−30	−40	−27	−115	34
Nordli I	34	*29*	22	6	−3	10	−79	26
Nordli II	16	*11*	1	−2	−15	−11	−43	14
Brundtland I	20	*15*	5	2	−12	−7	−39	14
Willoch I	36	20	19	12	5	*−11*	−36	20
Willoch II	41	24	24	*16*	*9*	*−7*	−31	22
Willoch III	22	12	8	*−1*	*−2*	*−6*	−48	14
Brundtland II	30	*20*	16	7	6	2	−41	17
Syse	44	45	30	*29*	*30*	*16*	−15	30
Mean Absolute Distance	23	17	14	16	19	20	50	23[a]
When in Government	—	14	13	8	10	12	—	11[a]
When in Opposition	23	22	14	20	24	25	50	25[a]

Note: Entries represent policy distances between party and government positions. Positive scores indicate that the government was to the right of the party, negative scores the opposite. Italicized figures represent parties in government. Mean policy distances for all governments are 11.2 for governing parties and 24.8 for the opposition, and for minority situations (excluding Nordli I and Brandtland I) are 10.3 for governing parties and 24.0 for the opposition.

[a] These are averages of the row means; since the *N*s differ, these values are not equal to the grand mean of the table entries.

geois coalitions have to draw up a separate document. Consequently, the coalition declarations may be much more carefully drafted and negotiated.

JOINT EFFECTS OF POLICY AND CONSTRAINTS

We conclude our empirical analysis with a more formal test of the joint effects of policy preferences and institutional constraints on cabinet formation between 1961 and 1990. We thus examine the predictive (or rather, postdictive) adequacy of various policy-based coalition theories under the applicable constraints. Table 7, column 1, reports the number of arithmetically possible coalitions (excluding the null coalition) in each bargaining situation, whereas column 2 gives the number of feasible coalitions under constraint. Only in 1969 and 1972 have the available options been sharply constrained—in both cases, in highly time-specific manners (the precommitment to continue the Borten coalition and the constraints arising from the EC controversy, respectively). The remainder of Table 7 reports the relative predictive success of four different solution concepts, representing different varieties of policy-based coalition theory. Thus, we pit viability (core theories) against the majority criterion, and ordinal orderings (connectedness) against cardinal ones (range). Column 3 represents Axelrod's (1970) hypothesis of minimal connected winning coalitions in ordinal policy space. In column 4, we substitute a more demanding cardinal policy measure—the winning (majority) solution with the smallest policy range (Leiserson 1966). Columns 5 and 6 represent the policy core. In column 5, we simply ask whether the core (median) party is included in the cabinet coalition, whereas column 6 tests the more radical and parsimonious proposition that if parties are purely policy seekers, the core party should be able to govern alone (Laver and Schofield 1990). This column hence reflects the minimal-range core. Our tests thus match two versions of the majority criterion against two versions of core predictions.

Since different theoretical propositions yield prediction sets of very different sizes, we consider the efficiency, as well as the success, of each solution concept. Success (number correct/number of cases) refers to the proportion of cabinets correctly predicted by each solution concept. Efficiency (number correct/number of predictions), on the other hand, measures the proportion of all predictions (postdictions) that are correct. By considering predictive efficiency as well as success, we do not inappropriately favor "shotgun" propositions that are consistent with a large proportion of all possible outcomes.

None of the solution concepts in Table 7 performs

TABLE 7

Predictions of Policy-based Coalition Theories in Norway, 1961–90

	NUMBER OF COALITIONS		SOLUTION CONCEPT			
YEAR	ARITHMETICALLY POSSIBLE	FEASIBLE UNDER CONSTRAINT	MINIMAL-CONNECTED-WINNING	MINIMAL-RANGE-WINNING	CORE PARTY INCLUDED	CORE PARTY ALONE
1961	63	63	2	1	32+	1+
1963A	63	63	2	1	32	1
1963B	63	63	2	1	32+	1+
1965	63	63	2+	1	32+	1
1969	31	1	1+	1+	1+	—[a]
1971	31	31	2	1	16	1
1972	127	63	1	1	32+	1
1973	255	255	2	1	128+	1+
1976	255	255	2	1	128+	1+
1977	63	63	2	1	32+	1+
1981A	63	63	2	1	32+	1+
1981B	127	111	2	1	48	1
1983	127	111	2+	1	48+	1
1985	63	63	2	1	32+	1
1986	63	63	3	1	32	1
1989	63	63	3	1	32+	1
1990	63	63	3	1	32	1
Predictive success[b]	—	—	.176 (3/17)	.059 (1/17)	.706 (12/17)	.375 (6/16)
Predictive efficiency[c]	—	—	.086 (3/35)	.059 (1/17)	.017 (12/721)	.375 (6/16)

Note: A plus indicates that this set includes the coalition that actually formed.
[a] No such coalition feasible under constraint.
[b] Number correct/number of cases.
[c] Number correct/number of predictions.

superbly, but any one is an improvement over chance. Since a total of 1,457 solutions were feasible under existing constraints in the 17 bargaining situations, a random prediction in a random case would have only slightly better than a 1% chance of success. The solution concepts in Table 7 perform from less than 2 to about 30 times better. Note the dramatically greater success of core versus majority theories, a contrast already suggested by Table 5. As noted, only 3 out of 17 cabinets have been minimal-connected-winning, and only one of these was in addition minimal-range. The core party, on the other hand, has been in 12 out of 17 cabinets. Despite its superior predictive success, however, core party inclusion is hardly a *discriminating* prediction, because, in the absence of constraints, half of all feasible solutions qualify. Hence, the efficiency of this criterion alone is lower than that of any competitor. Yet, the 12 correct predictions are an improvement of approximately 40% over the 8.5 that would have been expected from chance (17 cabinets × 721/1,457).

If we substitute the more radical prediction of the minimal-range core (i.e., the core party alone), predictive efficiency rises dramatically, to a ratio of better than one in three. Yet this solution concept has the seemingly undesirable property of precluding any genuine (interparty) coalition by design. Thus, although Table 7 confirms the attractiveness of core theory, each of the two related solution concepts has its obvious limitations.

DISCUSSION

One of the weaknesses of conventional theories of government formation is their inability to predict coalitions that are either "too large" or "too small." Norway has long featured a pattern of "coalition avoidance," which has challenged conventional assumptions in this literature. We have examined whether more parsimonious theories and more refined measures could improve on this record. Specifically, we have tested whether institutional constraints or core-based policy theory could explain the Norwegian anomalies. Rather than relying on subjective policy measures, we have drawn the bulk of our data from content analysis of authoritative party and government policy documents and from the record of portfolio allocation.

Our results point to policy viability as a key to coalition bargaining. Policy viable coalitions are those that include the core party in a spatial voting game. Generically, a core party is sure to exist only in a one-dimensional policy space, where it corresponds to the party controlling the median legislator. Our results confirm the salience of the left–right dimension in Norwegian party politics. While minority cabinets have been numerous, cabinets without the core party have been relatively rare. Yet, in some cases issues cross-cutting the left–right dimension, such as abortion and EC membership, have clearly contributed to coalition breakdown or avoidance. Our predictions also fail to capture the depth of the socialist vs. nonsocialist division. Coalitions between Labor and centrist bourgeois parties (particularly the Liberals) are predicted in many of our models, but such coalitions have never materialized.[17]

Bargaining constraints and strategic party behavior explain much of this discrepancy. Norwegian parties enjoy a permissive institutional environment, and the constraints they face are frequently of their own making (i.e., their own precommitments). Occasionally, as in 1969, such restrictions leave only one feasible cabinet solution. Yet, institutional factors have also had their more subtle influences. The disproportional electoral system, for example, has driven the smaller centrist parties into electoral coalitions, which have constrained executive coalitions with other parties, such as Labor. Also, given Labor's predominance, the nonsocialist parties have recognized the need for unity and the electoral costs of squabbling (Strøm n.d.). The impossibility of early parliamentary dissolution has fostered toleration of weak governments.

Policy Motivation and Coalition Theory

Policy-based coalition theory has come to dominate the study of cabinet coalitions in parliamentary democracies. While this study confirms the explanatory power of such models, we need to recognize the limited conditions under which they apply. Policy-based coalition theory rests on a number of restrictive assumptions, as we have discussed. We have shown that strong results can be generated where political institutions and policy space approximate these conditions. We cannot conclude, however, that similarly good fits would occur under bureaucratic government, under more prohibitive institutional constraints, or in a clearly multidimensional policy space.

Policy-based coalition theory faces three major theoretical challenges: (1) its applicability in multidimensional policy spaces, (2) its behavioral account of intraparty politics, and (3) its ability to capture the institutional environment of coalition bargaining and policymaking. The instability problems in multidimensional policy spaces, where the core is generically empty, have already been discussed. More permissive concepts, such as the Pareto set in weighted voting games, may yield equilibria in two-dimensional space but is subject to the same instability in three or more dimensions (Laver and Hunt 1992, 96–101; Laver and Schofield 1990, 119–31). The analytical punch of policy-based coalition theory thus critically depends on a low number of policy dimensions or on other restrictions on the feasible policy space (Laver and Shepsle 1990). This is especially troubling in view of the fact that losing parties have an incentive to expand the number of policy dimensions precisely so as to generate instability (Riker 1986). It behooves proponents of policy-based coalition theory to demonstrate how such strategies can be contained.

Moreover, policy motivation is an awkward primitive assumption in coalition studies. Why should party leaders invest so much energy in the pursuit of goals that are essentially public goods (Laver 1981)? Ultimately, policy-based coalition theory must assume that party leaders are largely instrumental in their policy pursuit (Budge and Laver 1986). That is to say, they seek policy goals because they are constrained to do so by their voters and activists (Robertson 1976; Strøm 1990a). Extensions of policy-based coalition theory must examine these constraints and establish the extent to which voters or activists can induce policy pursuit among party leaders. This means capturing the effects of electoral competition on coalition behavior (Lupia and Strøm 1993; Strøm 1990b), and it implies systematic attention to the constraints of intraparty politics.

Agency theory provides a useful set of tools for such purposes. Institutionally, parliamentary government entails a sequential process of authority delegation. The legislature delegates authority to a team of cabinet members who in turn oversee civil servants responsible for policy implementation. Such delegation regimes can be characterized by the degree of control retained by the respective principals (i.e., legislative parties) over their agents (ministers and, ultimately, civil servants). Conventional policy-based coalition theory implicitly assumes that party leaders retain effective control of the policy process, an assumption questioned by Laver and Shepsle (1990). Though the Norwegian evidence leads us to be optimistic about the feasibility of effective delegation and enforcement, future work on coalition bargaining needs to examine parliamentary delegation regimes much more systematically.

Policy-based coalition theory expresses an optimistic view of representative democracy. If voters care about public policy, we would be reassured to know that political parties were constrained by the same considerations in their coalition bargaining. As we have seen, demonstrating this linkage is no small matter. Nonetheless, it is a task eminently worth its while.

APPENDIX: PARTY DOCUMENTS AND PROCEDURES OF ANALYSIS

Party Programs

> Norwegian political parties regularly issue lengthy policy documents, frequently approaching one hundred pages. Most parties distinguish between two documents: programs of principle (*prinsipp-program*) and programs of action (*handlingsprogram*). The former are statements of ideology and long-term objectives, adopted by national party congresses for indefinite terms. Our focus here is on programs of action, which are

more concrete and contain more specific pledges. These programs, which are specifically tied to the four-year parliamentary term, are normally issued early in the election year.

Programs of action have become progressively longer, and the largest parties (Labor and the Conservatives) tend to issue the lengthiest programs. Program format has become increasingly standardized across parties. Most party programs begin with a brief statement of principles (a synopsis of the program of principle) and proceed to a fairly conventional set of issue areas, such as economic policy, foreign affairs, national security, national health, social welfare, and education. Some newer parties have published more unconventional programs. In its first electoral contest (1973), Anders Lange's party thus produced only a one-page legislative program. Likewise, the Socialist Electoral Alliance of 1973 issued a nontraditional document—a joint electoral declaration by its three component parties. These parties' subsequent programs have become lengthier and more conventional.

In our analysis, the programs of the Labor party, the Conservatives, the Christian People's party, the Center party, the Liberals, and the Progress party have been content-analyzed. In addition, we have created a category called the Socialist Left, which since 1977 is identical to the party by that name. For previous years, this category represents the Communist party (up through 1957), the Socialist People's party (1961–69), and the Socialist Electoral Alliance (1973). Each represents the only parliamentary left-wing party in its period of inclusion. (In 1973, the 16 members elected on the slates of the Socialist Electoral Alliance technically represented three different party organizations. However, since these parties presented a joint manifesto, the multiplicity of party affiliations presents no problem for our analysis.) The analysis includes all programs from 1949 through 1989. The 1945 election has been deleted, since the parties that year presented a joint platform for postwar reconstruction. Only two postwar parliamentary parties have been omitted from the analysis: the Liberal People's party, whose lone representative in the 1973–77 Storting had no bargaining power in any cabinet crisis, and for the 1989–93 period, independent representative Anders Aune, elected on the slate Folkeaksjonen Fremtid for Finnmark (Popular Movement for Finnmark's Future), a regional list.

Government Declarations

The documents analyzed are generally government declarations given at the time of cabinet formation. Two problems should be noted. First, some cabinets have presented no declaration upon taking office, due to the fact that governments fail to resign and thus be formally reconstituted in many situations where this would be customary in other parliamentary democracies. Thus no government declaration was presented between 1951 and 1963, during which period we count five consecutive Labor governments. Over the whole postwar period, the 13 government declarations fall considerably short of the 23 cabinets that have been formed. In cases of missing declarations, we have substituted the government's first throne speech. The only government omitted is Gerhardsen VI (1963–65), since Gerhardsen presented no new declaration when he returned to power after Lyng's four-week interlude. Also, the government declaration of Brundtland III was not yet available when this analysis was conducted.

Second, even some governments that issued ordinary declarations present problems in the analysis, since their declarations have been (mercifully) short. When Oscar Torp replaced Einar Gerhardsen as prime minister in 1951, he gave a declaration of no more than 11 codable sentences. When Odvar Nordli acceded to the premiership in 1976, his declaration was only one sentence longer than Torp's. And even when Nordli gave way to the more loquacious Gro Harlem Brundtland in 1981, the output was no more impressive than 35 sentences representing 12 coding categories. These taciturn administrations stand out in our analysis among the most conservative Labor governments. Their scores presumably largely reflect their stress on continuity with previous Labor administrations. As noted in the text, the most truncated of these documents have been deleted from some analyses.

Analysis Procedures

For the content analysis, each manifesto was broken down into individual sentences, or quasi sentences, which were then assigned to one and only one substantive coding category. In accordance with saliency theory, we count the relative frequency of the coding categories as evidence of their relative importance (Budge, Robertson, and Hearl 1987). The coding was performed by two graduate students at the University of Bergen. Informal checks showed high degrees of intercoder reliability. The categories through which the Norwegian party programs were sifted are identical to those of the larger study, except that one category (no. 415) and a small number of subcategories were added to adapt the coding scheme to special Norwegian policy concerns. The resulting coding scheme contains 55 substantive categories (plus 10 subcategories) within seven broad domains: international relations, freedom and democracy, government, economy, welfare and quality of life, fabric of society, and social groups.

Since most subcategories proved numerically insignificant, many were reaggregated in the final analysis. Each coding category is identified by a three-digit number (four-digit for subcategories), where the first digit identifies the domain to which it belongs. The analysis of the 1989 election and the Syse government was done separately and is not strictly comparable to the previous results. The subcategories that had been created for the original analysis were, in these cases, collapsed. Most subcategories had proven to be very thinly populated. With few exceptions, they had been reaggregated for the final analysis of the party manifestos (Strøm and Leipart 1989). The frequencies of the coding categories in the 1989 documents were, contrary to the previous procedure, converted into percentage terms *before* the computation of spatial positions. This may have caused slight rounding errors compared to the previous results. These changes in coding procedures make no substantive difference for the spatial analysis presented here, however.

The Left–Right Scale

Our left–right scale was constructed by subtracting the aggregate frequency of left-wing statements in each document from the aggregate frequency of right-wing statements. In calculating the left–right score of each party or government, we proceeded as follows. First, each coding category was represented as a percentage of all programmatic sentences in the document in question. Then, the scores (in percentage terms) for all the right-wing categories were added up. The left-wing categories were similarly aggregated. To get the final left–right score, we simply subtracted the aggregate percentage of the left-wing items from that of the right-wing ones. Thus, left–right scores vary between hypothetical bounds of 100 and −100, and high scores represent right-wing positions. The coding categories employed in the cross-national left–right dimension are, on the left, 103, 105–7, 204, 304, 403–4, 406, 412–13, 501–2, 602, and 604, and on the right, 203, 305, 401–2, 407, 410–11, 414, 505, 601, 603, and 605–6. See Laver and Budge 1992 for an identification of these coding categories.

Notes

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1. We use the terms *cabinet* and *government* interchangeably.

2. Until 1959 the Center party was known as the Farmers' party.

3. Willoch I (1981–83) nevertheless enjoyed consistent parliamentary support from the Christian People's party and the Center party.

4. Strictly speaking, the core party or parties have no incentive to include other parties in the coalition. However, as core parties should be able to insist on their most preferred policy position even in coalition with other parties, there should also be no policy cost to broadening the coalition.

5. Models of institutional constraint can also be used to generate stable solutions in multidimensional policy spaces. This, however, will not be our approach here.

6. There have been efforts in the emerging noncooperative coalition literature to model the bargaining process more explicitly (e.g., Austen-Smith and Banks 1988; Baron 1991, 1993; Lupia and Strøm 1993). Though this approach is promising, such works remain few and highly stylized.

7. In March 1971, Conservative president Bernt Ingvaldsen was given the task of exploring the prospects for continued nonsocialist coalition government. It was obvious that even if successful, the deeply conservative Ingvaldsen would not be the new prime minister. Ingvaldsen's efforts led to an attempt by Kjell Bondevik, of the Christian People's party, to resurrect the bourgeois coalition. When Bondevik's endeavor stranded, the task of forming a new cabinet passed to Labor leader Trygve Bratteli. During this crisis, the leaders of all parliamentary parties were consulted by the king.

8. Policy compromise between nonsocialist parties has often taken place in the context of parliamentary debate over the government's long-term economic program.

9. In Syse's cabinet, for example, the Center party and the Christian People's party each secured five portfolios, enough to give them a combined majority of one. This portfolio allocation represents the clearest deviation from parity, since Conservative members of Parliament (and voters) outnumbered those of the two centrist parties combined by a ratio of about three to two.

10. Beginning with the Willoch I government in 1981, the former ministry of Church and Education was divided into two separate departments. Yet until 1989, all matters dealing with religious affairs and primary and secondary education (the principal concerns of the Christian People's party) were still under the jurisdiction of the ministry bearing the original name. In the Syse government of 1989, separate ministries of Church and Education were established. Note, however, that the Christian People's party gained both portfolios in this coalition. We have therefore counted these two portfolios as if they had remained one.

11. Grønmo (1975a, 1975b) identifies two different versions of the left–right dimension, one urban and one rural.

12. The salience of the left–right dimension is evidenced by the fact that the programmatic items included in the scale collectively account for approximately 45% of all coded sentences in government declarations and a similar proportion of party programs. The salience of the left–right dimension is lowest for the Nordli and Borten cabinets (all under 40%) and highest for Gerhardsen II and Willoch I (both over 60%). Not surprisingly, the party most concerned with left–right issues is the Progress party (60–79%), whereas the Center party devotes them the least attention.

13. The conventional left–right ordering of Norwegian parties is reflected in the order in which they are listed in Table 4 and elsewhere.

14. The fact that the Christian People's party accounts for two-thirds of all "incorrect" pairwise rankings suggests that there may be a component of moral-religious traditionalism among our right-wing themes. The counterintuitive orderings relative to the Conservatives occurred between 1949 and 1977. In recent elections, the Christian People's party has moved decisively toward the left. Thus, in 1989 it was "incorrectly" positioned to the left of the Liberals, as well as to the left of the Center party.

15. Figure 1 allows only one government to be plotted per year, which creates only one problem for our purposes, namely, the two governments formed in 1981 (Brundtland I in February and Willoch I in October). Since Brundtland's cabinet was formed before the September elections and should not be related to party positions from that campaign, we have plotted it as if it were formed in 1980.

16. Interested readers might compare the policy range scores in Table 5 to policy distance in Table 6. Such a comparison would give an indication of the policy range *within* the cabinet relative to the distance between government and opposition.

17. Labor's long-standing anti-coalition stance, which we have not formally recognized as a constraint, helps explain these errors. Although the "Lib-Lab" coalition never has materialized in Norwegian politics, it was not a farfetched prediction in 1961–62. Both Gerhardsen and some Liberals seemed to favor a rapprochement. But the Liberals were hard pressed by the other nonsocialist parties to maintain a common front against the social democrats. Once the Liberals had joined the other bourgeois parties in the short-lived Lyng government, their hands were essentially tied for 1965 and 1969. Liberal withdrawal from this coalition after the electoral triumph of 1965 would have caused intense hostility from the other nonsocialist parties and, probably, from most Liberal voters.

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ADDRESSING THE POLITICAL EXCEPTION: MACHIAVELLI'S "ACCIDENTS" AND THE MIXED REGIME

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First, I will demonstrate that Machiavelli's conception of political phenomena is richer and more varied and conforms to an adequate understanding of political reality more closely than later, more "systematic," or "regular," conceptions often associated with the Enlightenment. Second, I find in Machiavelli strong grounds for resisting authoritarian claims that the variegated and unpredictable nature of political phenomena must be managed with unipartite and, especially, centralized practical political alternatives. I focus upon Machiavelli's frequent use of a particular word by which he refers to politically significant occurrences, accidente. *I argue that his employment of this word, especially in the* Discourses, *serves to accentuate his sensitivity to the irregular, nonsystematic nature of political reality—to the possibility of "exceptions." But I demonstrate that his practical response to this reality, is not a perpetually vigilant, all-powerful sovereign but is, rather, a far more moderate answer, the mixed regime.*

One of the most prominent characteristics of what is, for better or worse, referred to as "postmodernism" is a disenchantment with the Enlightenment's view of "nature" broadly speaking. Related literature exhibits what is of more interest to political scientists, a dissatisfaction with the Enlightenment's conception of *political* nature. Liberal political theory, in particular, has been vilified for its naive presumption of the regularity of political phenomena and its resulting inattention to "exceptions."[1] This call for a more sophisticated understanding of political phenomena may be justified. But is the practical response corresponding to this understanding necessarily of the potentially authoritarian kind associated with particularly centralized political alternatives? No. The recognition of the political realm as one that is characterized by the utmost variability and unpredictability need not imply that the only way to manage such a reality is with unipartite and executive-centered political fiats. For a model of how to combine a nuanced understanding of political nature and a less narrow or even extreme practical response to it, I suggest we turn not immediately to "postmodern" alternatives but rather to a writer who is arguably thoroughly modern—Niccolò Machiavelli.

What is the nature of political phenomena? Are they consistent or erratic? If the latter, what bearing does this have on political change? What forces come into play such that once a regime is constituted it does not last perpetually? Why should not consolidated regimes, at least, be expected to do so? Machiavelli was obsessed with these questions, and one of his many original contributions to political thought was his identification of the mixed regime as the most effective antidote to those irregular forces that confront and potentially bring down political entities. John Pocock goes so far as to name this confrontation between regime and unpredictable political nature after the great Florentine, on the grounds that Machiavelli has so shaped our modern understanding of political stability. Hence, the "Machiavellian moment" is "the moment in conceptualized time in which the republic was seen as confronting its own temporal finitude, as attempting to remain. . . stable in a stream of irrational events conceived as essentially destructive of all systems of secular stability" (1975, viii). Yet Pocock fails to ascertain adequately and in detail what constitutes the nature of these "irrational events." He is therefore in a somewhat disadvantaged position to study Machiavelli's response to them, namely, mixed government. Pocock too readily accepts the dichotomy of *virtù* versus *fortuna* as Machiavelli's sole paradigm of the human attempt to manage political instability (chaps. 6–7). In fact, the fortune–virtue distinction has become a kind of "set piece" in Machiavelli scholarship and, as such, has come to convey a sense of order that actually defies the spirit of Machiavelli's approach to the issue of contingency.[2]

A more fruitful approach, I suggest, is to look directly at the variegated phenomena that Machiavelli described as examples of threats to political stability and to focus precisely upon a word with which he frequently referred to them, *accidenti.*

The use of the term *accidente* in the intellectual world of Renaissance Florence was both a residue of medieval scholastic thought and a manifestation of a contemporary revival of Aristotelianism.[3] What was "accidental" was opposed, in Aristotelian or Thomist thought, to what was "substantial." Hair or eye color were "accidental" to the nature of a person; they had little or nothing to do with that person's substance, his or her "essence." In Machiavelli's day, the word continued to be used in this sense, particularly in political discourse. Something that happened *to* a regime (usually due to some extrinsic, not intrinsic cause) was generally referred to as an *accidente.*[4] *Accidente,* like our word *accident,* was a descriptive not an evaluative term. An *accidente* was not viewed as necessarily good or bad. Its defining characteristic was rather its unexpected quality; it was impossible or at best very difficult to foresee. To the extent that an *accidente* could and potentially would detrimentally affect a regime, or even cause its collapse, those concerned with politics needed to be alert to it, though not necessarily to make it their central focus.

Machiavelli somewhat altered the traditional conception of *accidenti.* By describing political phenomena often exclusively in terms of *accidenti* and by expanding the use of the term in ways that I will describe, Machiavelli made what was by definition external, extrinsic, and insubstantial into the very *core* of politics and hence the central focus of political thought. Yet Machiavelli, who is most famous as the author of a book entitled *The Prince,* actually resisted the temptation to confront the unpredictable nature of politics with a wholly executive-oriented response, instead advocating the mixed regime precisely because he thought it could best withstand the *accidenti* that inevitably arise from time to time.

In addition, Machiavelli was quite eager to give examples of *accidenti* that have occurred in history and to demonstrate what kind of actions or precautions have managed each of them successfully. If no success had been achieved in the particular historical context of the *accidente* in question, Machiavelli provided his own proposed solution.[5] As he explained in the *Discourses:* "It is an easy thing to whoever examines past things diligently to foresee future things in every republic and to take for them the remedies that were used by the ancients, or, if they do not find any that were used, to think up new ones through the similarity of the accidents" (1.39).[6]

The task of this paper is to explain more fully Machiavelli's conception of political phenomena through an analysis of the variety of *accidenti* as they appear in the *Discourses,* his disquisition on mixed regimes or republics, and to decipher why he thought that such a regime was best equipped to deal with them.

THE NATURE OF *ACCIDENTI* AND HOW TO MANAGE THEM

Machiavelli's work is not completely free from the conventional scholastic use of the term *accidente.* The word often connotes for him a simple happenstance, an occurrence, or an external event requiring little commentary on how it should be treated by a regime. For instance, he refers to floods and pestilence, without much elaboration, as *accidenti* (2.5). He applies the same term elsewhere to a simple expression of public opinion (1.58). In *Discorsi* 1.46, Machiavelli speaks of some unspecified *accidente* that might lift up a government from a position of servitude to one of freedom. While he does not elaborate here, this example leads us to some of Machiavelli's more interesting uses of the word *accidente.*

An event that culminates in a political entity's achievement of freedom is also referred to as an *accidente* in 1.16, when Machiavelli speaks of some *accidente* that might liberate a people who have been living under a prince. But here he has a particular historical instance in mind—Rome's expulsion of the Tarquin kings. This is no ad hoc occurrence like a flood or a plaque, which would owe its appearance only to a freak of nature, or an "incident" narrowly defined. It is, on the contrary, an act of will carried out by a political body. And along with giving a complicated example of an *accidente,* Machiavelli describes the means of dealing with it—in this particular case, not how to prevent it or ward it off, but rather the means to put it to good use. To take advantage of the timely event of the Tarquins' demise, Machiavelli advises that regimes that find themselves in similar postprincely situations do as the Romans did and "kill the sons of Brutus"—in other words, eliminate those who sympathize with the old regime and who might seek to reinstate it.

This advice is reminiscent of Machiavelli's treatment of the same situation in chapter 5 of *Il Principe.* Machiavelli again refers specifically to an *accidente* that will change a position of submission into one of liberty—in this case, the newly won freedom of a formerly conquered state. Here the historical example is Pisa's liberation from Florence. But the advice proffered is not how to transform such an *accidente* into a long-term good, but rather how a conquering regime can guarantee that such an *accidente* never comes about in the first place. If a territory acquired through conquest was originally a republic, Machiavelli advises that its inhabitants be scattered and their civic bonds severed so as to make the republic's reconstitution impossible. This, however, is difficult to achieve, he says. If the territory was originally a principality then the task is easier. All that is necessary is to wipe out the family of the hereditary ruler. Even though the advice given here (concerning *accidenti* that bring liberty to a subjugated state) is different from that set forth in the *Discorsi* in being officially addressed to a prince, it is still useful to mixed regimes. For Machiavelli, mixed regimes must be expansionist and imperialist in order to last (2.19) and so will be just as interested as princes in preventing subjugated territories, whether former monarchies or republics, from gaining their freedom through some *accidente.*

From these examples of *accidenti* that bring freedom, we may begin to infer a general pattern governing Machiavelli's treatment of the term and the political reality with which it corresponds. Machiavelli relates a historical instance, identifying it as an *accidente,* then proceeds to explain how such an occurrence was dealt with, correctly or incorrectly, within its own specific historical context. He does this in order to demonstrate to the reader how to avoid such a situation, if dangerous, or profit by it, if fortuitous. If no recourse was taken or the wrong method was employed in history, Machiavelli proposes a correct one.

But there is also another ever-important element to be considered: the element of *time*, which Pocock is correct in emphasizing repeatedly. It is just as crucial to know *when* to act as to know *what* to do in the attempt to manage *accidenti*. Machiavelli stresses time and again in the *Discorsi* the necessity for governments, with future unforeseen events in mind, to apply due measures at an appropriate time. He expresses this most concisely however in a passage from chapter 3 of *Il Principe*: "They not only have to have regard for present troubles but also future ones, and they have to avoid these with all their industry because, when one foresees from afar, one can easily find a remedy for them but when you wait until they come close to you the medicine is not in time because the disease has become incurable."[7] The *accidente* that prompts this particular insight is the potential invading or influencing of a weaker territory lying adjacent to one's regime, by a rival regime at least as powerful as one's own. The historical precedent of this is established, according to Machiavelli, by the Greeks, who allowed the Aetolians to be conquered by the Romans and were themselves subsequently conquered by the Romans. The "remedy" for this particular *accidente*—the precaution informed by "foresight" that would prevent it from arising—is to make one's regime "head and defender of the neighboring lesser powers" (chap. 3). The Romans always forestalled the arrival of the *accidente* just described by fighting rival powers in the territory of their satellites and rarely in the area of Rome itself: "The Romans, seeing inconveniences from afar, always found remedies for them and never allowed them to continue to escape war, because they knew that war may not be avoided but is deferred to the advantage of others. So they decided to make war . . . in Greece in order not to have to do so in Italy" (chap. 3).[8]

Thus, merely to *recognize* an *accidente* is not enough to guard against its danger or profit from what it brings. Nor is it sufficient to know what steps are to be taken. A response of either type must occur at an appropriate time in order to be effective. For example, in the passage about the Tarquins cited earlier, Machiavelli emphasizes that the reason the Romans were able to benefit from their expulsion is the fact that the city was still young and uncorrupted. Had the termination of the monarchy come at a later date, either Rome would have been too corrupt to take measures to preserve liberty or such measures would have been ineffectual if applied.

But if *accidenti* can be managed only if seen from afar, then they are almost impossible to combat because, by definition, it is almost always impossible to foresee them. As Machiavelli himself rather pessimistically points out: "If how human things proceed is considered well, it will be seen that often things arise and accidents come about which the heavens altogether have not wished to be provided against. And if what I say happened at Rome (where there was so much virtue, so much religion, and so much order), it is no marvel that it should happen much more often in a city or a province that lacks the things said above" (2.29). Most of the time, however, Machiavelli presumes that *accidenti* can indeed be seen before their development. In fact, the method of dealing with them that I have elucidated as Machiavelli's fully depends upon such foreseeability.[9]

The solution to the foresight problem is crucial, for Machiavelli describes quite dramatically the consequences of not addressing problems in an *early* fashion as well as with a *proper* remedy. In the *Discorsi*, Machiavelli treats the *accidente* of Florence's loss of Pisa in more detail than in the example from *Il Principe* just described. Machiavelli explains, in terms of what might be called a chain reaction, the course of events triggered by Florence's mistake of allowing a great power to gain influence in its neighboring territory. What is described as an *accidente* in the title of 1.39—France's role in Pisa's rebellion—obliged Florence to make war, which in turn forced it to spend a lot of money, which increased taxes, which upset the populace, who overturned the city government, which thus could no longer prosecute the war, which finally led to Florence's loss of additional territory (ibid). The breathless prose and assumption of rigid causality with which he presents this course of events give the reader the impression that the successive occurrences were inevitable once the initial *accidente* came to be. The only alternative offered that might "break the chain," so to speak, is the possibility that the people might realize that their response to the government was counterproductive. They might give the government the requisite leeway—which in fact they eventually did, albeit too late to salvage the war and save the territories.

Although he does not mention the word *accidente* when discussing the events surrounding the Agrarian Laws in Rome, Machiavelli does emphasize foresight and speaks with an air of inevitability about the consequences. In 3.24, Machiavelli relates how in the wake of the Agrarian Law controversy, the habit of calling for the prolongation of offices—for the tribunes by the people and for the consulate by the Senate—led to the prolongation of military commands, and to the creation of proconsuls (which became more necessary as Rome's armies traveled farther abroad). As a result, only a few men gained military expertise and these very few, because of the length of their terms, held more sway with the army than did the Senate. This was the precedent that Caesar was to follow. This chain of events, which stretches over centuries, seemingly could have been avoided only had the original incident over the issue of offices been dealt with more appropriately. In this regard, Machiavelli laments, "If these things had been known well from the beginning, and proper remedies produced for them, a free way of life would have been longer" (ibid.).[10]

The chains of events that seem to follow specifically from ill-prepared-for *accidenti* are also described in terms that make them sound inevitable, successive, and nearly irreversible. Unless a republic has laws that prevent a rich citizen from gaining undue political influence and friends through money or protection, that citizen will be feared by the common citizens and treated deferentially by officials until he is virtually unassailable. The only choices that remain for those uncomfortable with such a situation are, on the one hand, the use of violence to bring such a figure down or, on the other, the suffering of servitude in silence. Such is "the order of these accidents" (*l'ordine di questi accidenti*) (1.46). The implication of this statement is that without the establishment, at the very beginning, of the specific law that Machiavelli advocates to prevent the ascent of such a citizen, ill events will follow in the order described with little chance of interruption. (The only possible escape from the chain entails violence and hence a new set of dangers.) The law could not be instituted at some *later* point in the scheme, after the initial appearance of the *accidente* of a rich person's rise to prominence, because he will then hold so much weight with the political institutions as to forestall such legislation.

This dire course of events is akin to an *accidente* discussed earlier in book 1. There the advice given is phrased somewhat differently, however. Certain dangers result from a republic's bestowing excessive honors upon its young nobles (1.33). The results are similar to the scenario just described, in that exemplary citizens rise to the ranks of petty tyrants. Machiavelli does not say that such actions should be prevented from the start, for he implies that it is almost inevitable—and perhaps good, to some extent—that a regime should tend to treat its most talented youth accordingly, occasionally bending the rules for them. Nor does he maintain that ruin is inevitable once this *accidente* has been set in motion because the *accidente* will be mistaken for something good and not dangerous. This example is interesting because Machiavelli, who is famous for favoring impetuosity over inaction,[11] uncharacteristically advocates doing nothing in this situation: "Since it is difficult to recognize these evils when they arise—the difficulty caused by a deception that things make for you in [the] beginning—it is a wiser policy to temporize with them after they are recognized than to oppose them; for if one temporizes with them, either they are eliminated by themselves or at least the evil is deferred for a longer time" (ibid). In the previous example, action was deemed impossible due to the further danger it might engender; but outright *inaction* was not expressly advocated as it is here.

As with the previous two, this episode is characterized by Machiavelli as an *accidente* that arises from causes internal to a regime. The *accidente* is not internal in any *essential* or *substantial* sense but merely in the sense that it arises within the domestic realm, rather than from the external realm, as do those associated with foreign affairs or natural disasters.

Yet Machiavelli does speak elsewhere of *accidenti* that develop as a result of a regime's intrinsic nature. In book 3 he declares that "because in everything . . . is hidden some evil of its own that makes new accidents emerge, it is necessary to provide for this with new orders" (3.11). Yet this use of *accidenti* seems significantly different from what I have established as the usage common in Renaissance Florence and even with the separate use that I have associated with Machiavelli so far. This use is not Aristotelian (as were those in the earliest examples); nor is it "Machiavellian" in the same sense as are the bulk of the examples I have been presenting. It is instead Polybian.

Machiavelli has taken this traditionally Aristotelian word and given it here a use derived from Polybius' analysis of the mixed regime. As Polybius wrote in book 6 of his *Histories*, "Just as rust eats away iron, and wood-worms or ship worms eat away timber, and these substances even if they escape danger are destroyed by the processes which are generated within themselves, so each [simple] constitution possesses its own inherent and inseparable vice" (1979, 310). Rather than something external to the regime, like a foreign invasion or a flood, or even something internal to a regime but not necessarily related to its substance as such, like the Roman expulsion of the Tarquins, the vice spoken of by Polybius and the *accidente* spoken of in this instance by Machiavelli are generated by the very core of a regime, spawned by its very essence.[12] As I will demonstrate in my analysis of Machiavelli's theory of the mixed regime, such use of the term *accidente* is rare in the *Discorsi*, and the question of just how a mixed regime should manage *accidenti* in general must wait for the analysis that lies ahead. Before embarking upon that discussion, however, a summation is in order.

I have by no means provided an exhaustive list of Machiavelli's *accidenti*. There are more examples within the *Discorsi*, as well as more to be considered in the following sections. What can be concluded at this juncture is that Machiavelli applied the term *accidenti* to many different occurrences. The crucial similarity among these lies in Machiavelli's belief that for the sake of the longevity of a regime, all such *accidenti* need to be managed. In a very simple way, Machiavelli is quite radical. He speaks of politics extensively—almost exclusively—in terms of that which is conventionally considered remote. And he speaks of preventing, forestalling, or putting to good use that which is normally thought to be unpredictable or uncontrollable. These are indeed peculiar uses of the word *accidenti*—uses that must have seemed strange to minds that had been psychologically and philosophically conditioned by the works of Aristotle and St. Thomas to consider whatever was referred to by such a word as, if not insignificant, than certainly not fundamental.

To be more specific, except in those cases where they appear as authorless occurrences (floods, etc.) and despite some ambiguity regarding the full extent to which he thought they could be handled, Machiavelli generally describes the appropriate measures needed to be undertaken to manage *accidenti* and emphasizes the necessity of applying them at a crucial time. *Accidenti* appear as actions on the part of one's regime or as actions on the part of someone else's regime. They can be an event brought upon one's regime from without or from within, and they can win freedom or ensure enslavement. If ignored, they may lead to a chain of uncontrollable events; if properly temporized, they may fade away. *Accidenti* may bring good as well as ill. Indeed, the destructive quality of *accidenti* can be overemphasized, resulting in the neglect of the full range of Machiavelli's employment of the term and of the conceptual apparatus that accompanies it. In 2.29 Machiavelli gives a list of dangerous *accidenti* that fortune inflicted upon Rome: it caused Rome to send inappropriate ambassadors to the Gauls, ordained that Rome do nothing about the oncoming invasion of the Gauls, exiled Camillus, did not let Rome appoint a dictator when needed, and so on. Yet (or so Machiavelli claims) these afflictions did not ruin Rome. On the contrary, they made Rome stronger and allowed it to attain the greatness for which it was destined. It is this idea that *accidenti*, even dangerous ones, are good for regimes that leads us to Machiavelli's most peculiar use of the word. Specifically, I want to ask how the theory of *accidenti* relates to the subject of the mixed regime and Machiavelli's preference for it.[13]

ACCIDENTI AND THE MIXED REGIME

Machiavelli shares with his predecessors who favored the mixed regime (most notably, of course, Aristotle and Polybius) the belief that stability and longevity are reliable measures of such a regime's success.[14] In a famous passage from the *Politics* that we have seen Machiavelli echo, Aristotle claims that "to know the causes which destroy constitutions is also to know the causes which ensure their preservation" (5.8.1).[15] Further on, he states: "The maintenance of a constitution is the thing which really matters. A state may last for two or three days under any kind of constitution; Legislators should therefore direct their attention to the causes which lead to the preservation and the destruction of constitutions . . . and on that basis they should devote their effort to the construction of stability. They must be on their guard against all the elements of destruction" (6.5.1). The primacy of stability and longevity is expressed in Polybius as well: "The test of true virtue in a man surely resides in his capacity to bear with spirit and with dignity the most complete transformations of fortune, and the same principle should apply to our judgment of states" (1979, 302). Machiavelli, as is widely known, incorporates the respective theories of the mixed regime from these two authors. But he undertakes a more extensive treatment than they do of "the causes that destroy constitutions," or "the transformations of fortune"—here specifically identified as *accidenti*—and the means to address them. Machiavelli does not confine the subject to a book 5 or book 6 of his study of mixed regimes but, rather, makes it central to the whole of his *Discorsi*. Defying chronology, let us turn first to Polybius' account of the benefits of mixed government regarding stability, and Machiavelli's treatment of it, before considering his treatment of the Aristotelian mixed regime.

As has been well documented,[16] Polybius puts forth the theory that the mixed regime was adopted by Rome and Sparta to break the "natural" cycle of degeneration that all simple regimes undergo indefinitely.[17] This cycle of political revolutions is "the law of nature," according to Polybius; it grows "from natural causes, and will pass through a natural evolution to its decay" (1979, 309–10). Apropos of the passage cited earlier from Polybius concerning the vices inherent in all simple substances, the inferior regimes (tyranny, oligarchy, and mob rule) are inherent within the good ones (kingship, aristocracy, and democracy). According to Polybius, Lycurgus of Sparta

> foresaw this, and accordingly did not make his constitution simple or uniform, but combined in it all the virtues and distinctive features of the best governments, so that no one principle should become preponderant, and thus be perverted into its kindred vice, but that the power of each element should be counterbalanced by the others, so that no one of them inclines or sinks unduly to either side. In other words the constitution should remain for a long while in a state of equilibrium thanks to the principle of reciprocity or counteraction. (pp. 310–311)

Lycurgus attained "the best of all existing constitutions" through his "powers of reasoning" and his "foresight." For Polybius, there is another way to achieve the same "result" or "goal." Rome became a mixed regime through the "lessons which misfortune teaches." It did this not "by means of abstract reasoning, but rather through the lessons learned from many struggles and difficulties; and finally, by always choosing the better course in the light of experience acquired from disasters" (p. 311). Polybius makes no clear judgment as to whether the Roman or the Spartan way of attaining the mixed regime is better.

While adopting much of Polybius' account of the rise of the mixed regime in Sparta and Rome, there is also much with which Machiavelli dispenses. For instance, he generally accepts Polybius' theory of cycles, except for their cause and ultimate result. Polybius attributes the genesis of the cycle to *nature*, Machiavelli to *chance* (1.2).[18] Polybius maintains that the cycle would go on indefinitely, Machiavelli that the regime would probably become "subject to a neighboring state that is better ordered than it" (ibid.).[19] These alterations demonstrate Machiavelli's dissatisfaction with Polybius' overly determinist explanations of political change. If political regimes undergo change in the manner of the cycles because of chance, there is less emphasis on a preordained order and more room for human action, preparation, and so on than if such change were governed by nature as such. This rejection of Polybius' natural causes also illustrates how uncharacteristic of Machiavelli is the "Polybian" example of an *accidente* at 1.33. The idea that a regime will "degenerate" by its very "essence," that its downfall will occur "naturally," is not usually found in Machiavelli. The second departure from Polybius further bears this out. For Machiavelli, an external force—another regime—is the last word on the degenerating regime: there is no perpetual spin of the cycle. This shift emphasizes that *accidenti* are generally external (or at least, if internal, then internally nonessential) phenomena and hence paradoxically, both less reliable and more controllable. Thus it is on the issue of the very nature of political phenomena—*accidenti,* if you will—that Machiavelli breaks with Polybius.

Accidenti are also the source of Machiavelli's other major departure from Polybius. Machiavelli at first appears to accept Polybius' neutrality toward the two ways a political body can become a republic and even appears to lean in favor of the Spartan model. He writes that

> either in their beginning or after not much time, [some cities] were given laws by one individual alone and at a stroke, like those that were given by Lycurgus to the Spartans; some had them by chance and at many times, and according to accidents, as had Rome. So, that republic can be called [happy] whose lot is to get one man so prudent that he gives it laws ordered in a mode that without having need to correct them, it can live securely under them. And Sparta is seen to have observed them more than eight hundred years without corrupting them or without any dangerous tumult: and on the contrary that city has some degree of unhappiness which, not having fallen upon one prudent orderer, is necessitated to reorder itself by itself. (1.2).

But when actually discussing Rome, Machiavelli shows how its "infelicitous" transformation into a mixed regime is *better* than Sparta's "felicitous" birth as a mixed regime because of Rome's relationship to *accidenti.* Rome was established as a kingdom, soon became a tyranny, but through the *accidente* of the Tarquin expulsion, it salvaged the element of kingly power in the consulate, rather than degenerating into another type of regime. To some extent, the cycle is "absorbed" by the republic:

> Having at once ordered two consuls there who stood in the place of the King, [the Romans] came to expel from Rome the name, and not the kingly power; so, since there were the Consuls and the Senate in that republic, it came to be mixed only of two qualities out of the three [of mixed government], that is, the Principality and the Aristocrats. It remained only to give a place to the popular government; hence, when the Roman nobility became insolent, . . . the People rose up against it; so as not to lose the whole, [the nobility] was constrained to yield to the people its role. . . . And thus was the creation of the Tribunes of the plebs, after which creation the state of that republic came to be more stabilized, since all three kinds of government there had its role. (1.2).

Why was Rome's way to a mixed regime better than Sparta's? Based on what we have learned so far of Machiavelli's conception of political phenomena and his proposed methods for dealing with it, the Roman model is more likely to occur in the world of politics. It is also the more likely to last. A regime that is built by *accidenti* will be more adept at managing *accidenti.* Lycurgus' Sparta—which sought to escape what Pocock calls "contingency" and prosper indefinitely as having been designed by a "timeless agency"—is, in a Machiavellian sense, unreal.

As Pocock observes,

> The goal defined by Polybius and achieved by Lycurgus may still be to escape from time and change, but there are circumstances in which citizens move toward this goal through the efforts of their own time-bound selves. The interesting case [for Machiavelli] is not that of Sparta, where a formula for timelessness was written in a single moment by a legislator virtually independent of time; it is that of Rome, where the goal was achieved . . . by the disorderly and chance governed actions of particular men in the dimension of contingency and fortune. Men who do this escape from *fortuna* by the exertion of a *virtù* which is their own and not that of a superhuman legislator. . . . The legislator-prophet is an even rarer figure in the *Discorsi* than in the *Principe.* (1975, 190, 192)

Pocock's position here has much to be said in its favor. There are, however, several problems with this interpretation, of which I shall describe one.[20] Pocock is indeed correct regarding the reason for Machiavelli's preference of Rome over Sparta, but he is correct on the basis of the wrong evidence. Machiavelli does maintain, paradoxically, that greater stability and longevity can be achieved by exposing a regime to "the dimension of contingency" than by its seeking refuge in the realm of timelessness. But this "dimension" is characterized in the passages in question by *accidenti,* not by the *fortuna–virtù* opposition upon which Pocock is fixated. According to Machiavelli, a constitution, though not born perfect, can be made so through "the occurence of *accidents*" despite (or perhaps because of) the fact that such a way is dangerous: "But it is indeed true that [such regimes] will never order themselves without danger, because enough men never agree to a new law that looks to a new order in a city unless they are shown by necessity that they need to do it. And since this necessity cannot come without danger, it is an easy thing for the republic to be ruined before it can be led to a perfection of order" (1.2).

Machiavelli tells us in the previous chapter that necessity inspires people to greater deeds than does choice (1.1), thus setting up the paradox: potentially dangerous *accidenti,* when properly handled, can improve a regime by causing it to renovate itself. Exceptional circumstances put a regime in danger, giving it the opportunity for even greater achievements. When we examine Machiavelli's prescriptions for how a mixed regime can maintain itself, we shall again see the advantage of Rome's constitutional coming-to-be over Sparta's: *accidenti* regularly give a regime the occasion to introduce the "new modes and orders" that every mixed regime needs to reinvigorate itself. As Aristotle points out: "The preservation of a constitution may not only be due to the fact that a state is far removed from the menace of any danger: it may also on occasion, be due to the very opposite. When danger is imminent, men are alarmed, and they therefore keep a firmer grip on their constitution" (*Politics* 5.8.8).

This brings us to Aristotle's theory of the mixed regime and Machiavelli's use of it. As we have seen, besides the theory of the cycles, what Machiavelli derives from the Polybian model of the mixed regime primarily concerns institutions. Polybius focuses almost exclusively on the formally political aspects of the Spartan and particularly the Roman government: the consulate, Senate, and tribunes as expressions of the monarchical, aristocratic, and democratic elements of political organization. But Machiavelli also incorporates the Aristotelian socioeconomic definition of the mixed regime and discusses at length the relationship between the *popolo* and the *grandi*—the common people and the nobility, the poor and the rich.[21]

It is not only the institutionally mixed nature of the Roman republic that allows it to confront *accidenti* (swallow them, so to speak) and improve itself as a result. According to Machiavelli, the socially mixed nature of the regime also allows it to do so. In fact it is the socioeconomic element that is ultimately responsible for precisely the institutional perfection previously noted. According to Machiavelli, "disunion" between the plebeians and the Senate actually caused "so many accidents" that what an organizer like Lycurgus had not done was accomplished by "chance" (1.2).

This theme is elaborated in the directly following chapter, "What Accidents Made the Tribunes of the Plebs Be Created in Rome Which Made the Republic More Perfect" (1.3). The *accidenti* referred to in the title are explained in the body of the chapter. The nobles initially treated the people well because they needed the people's support against the Tarquins. Yet after the Tarquins were expelled, the nobles began abusing the people. But again, "necessity"—a condition that is brought about by forces of chance such as *accidenti*—compelled Rome to come up with a replacement for the Tarquins, to keep the nobles at bay as they had. After "many confusions, noises, and dangers of scandals that arose between the plebs and the nobility," the tribunes replaced the Tarquins vis-à-vis the nobles as the consuls had replaced the kings vis-à-vis the people. In a later chapter, Machiavelli describes how the tribunes were in turn checked when they began to abuse their power (3.11).

Machiavelli derives from Aristotle the idea that the principal class components of regimes ought to be mixed together when constituting republics. In the *Politics*, Aristotle declares that simple regimes are not defined in terms of number (a democracy is not majority rule, nor oligarchy rule by the few) but, rather, in terms of the socioeconomic group that is dominant (3.8). The criteria by which Polybius defined regimes—the one, the few, and the many—are deemed merely "accidental" by Aristotle (3.8.6). They are not the definitive characteristics. Thus, Aristotle's mixed regime, the best and most stable regime—the *politeia*—blends the rich and the poor, the wealthy and the freeborn (4.8.8).

In the *Discorsi* 1.4 Machiavelli draws upon this formulation and seemingly extends it further. He restates Aristotle's position faithfully: "In every republic there are two diverse humors, that of the people and that of the great." But he goes on to write, shockingly, "All the laws made that are made in favor of liberty arise from their disunion" (1.4). This emphasis on *discord* might be read as a repudiation or at least a departure from the Aristotelian ideal. After all, in book 5 of the *Politics, stasis*—the ultimate threat to a regime, according to Aristotle—most often arises as strife over real or perceived economic inequality. But is Machiavelli really celebrating such strife with his emphasis on the *accidenti* that arise from discord? No. Upon closer examination, we see that Machiavelli never abandons Aristotle's criteria for what makes a healthy relationship between rich and poor, even if Aristotle considered that relationship a "blend" (*Politics* 4.8.8) and Machiavelli considered it a "disunion." Aristotle writes: "A properly mixed polity . . . should owe its stability to its own intrinsic strength, and not to external support; and its intrinsic strength should be derived from the fact, not that a majority are in favor of its continuance (that might well be the case even with a poor constitution), but rather that there is no single section in all the state which would favor a change to a different constitution" (*Politics* 4.9.10). As long as no group or social class is opposed to the regime itself, regardless of how it feels toward the other groups within it, according to Aristotle, the constitution is safe and the regime sound. Machiavelli fully adheres to this view, for (as mentioned earlier) he cites the consequences of the Agrarian Laws as the beginning of the end of the mixed regime in Rome. It was at this moment that the people tried to change the very nature of the constitution in favor of themselves and at the expense of the nobles by limiting land holdings and distributing conquered enemy land exclusively among the plebs (1.37). Additionally, Machiavelli makes clear that healthy Roman disunion rarely resulted in exiles, civil bloodshed, executions, or excessive fines (1.4). Machiavelli's commitment to the ideal that no group should be hostile to the constitution is in evidence when he advocates killing those who are involved in clan warfare (3.27, 30). Thus the disunion or discord that Machiavelli credits with causing *accidenti* that made the republic stronger is not a violation but a radical stepping forward of the Aristotelian theory of the mixed regime.

Machiavelli has very specific ideas about how this discord should be facilitated. I will point out only a few examples as they pertain to the topic of *accidenti*.[22] An important point for Machiavelli is the citizens' right to bring forth public accusations against even the most powerful figures because it (1) deters acts that *might* be committed against the regime, (2) leads to punishment of acts that are *actually* committed against the regime, and (3) provides an outlet for partisan discharge within legal boundaries. Machiavelli furnishes two examples of *accidenti* to demonstrate, on the one hand, the disasters that did *not* occur in Rome, which had a procedure for accusations, and on the other, the disasters that *did* occur in Florence where no such safety valve existed. We thus derive some further idea of what the difference between proper and dangerous discord might be to Machiavelli.

In Rome, when Coriolanus offended the people, had he not been required to answer the charges officially and publicly, Machiavelli assures us that he would have been killed by the mob. This *accidente* would have had devastating consequences that would have taken the form of the chain reactions illustrated earlier. His death at the hands of the mob would have signaled a private injury to the nobles, which in turn would have sparked fear, which would have called for defense, which would have required partisans, from which would have risen factions, and from such factions ruin would have followed (1.7).

In Florence, the very real *accidente* that arose because no means existed for making accusations involved the invitation to invade delivered to a foreign power, Spain, by a party that felt it could not challenge Soderini domestically. Machiavelli explains: "If such modes [of accusation had existed in Florence], either the citizens would have accused [Soderini], if he was living badly; and by such means they would have purged their animus without having the Spanish army come; or, if he were not living badly, they would not have dared to work against him for fear of being accused themselves" (1.7). As a rule, Machiavelli wryly observes, if one party in a republic calls in a foreign army, discord has become unhealthy.

Consider again the issue of discord and *accidenti*, Machiavelli prefers Rome to Sparta on the Aristotelian issue of social groups for the same reasons that were explained regarding the Polybian question of foundings and institutions. Even though Sparta and Venice were mixed regimes that lasted long without discord between their classes, the Roman mixed regime, fueled by discord, is superior, for Machiavelli, because its structure is more conducive and specifically *adaptive* to political reality. As Machiavelli demonstrates in 1.6, healthy civil discord and expansion go hand in hand. The energy created by the one must be directed toward the other. Roman discord, resulting from full inclusion of the people within the republican government, leads to Roman empire. Sparta, because it was small, well-defended, and tranquil, did not have to expand.[23] Yet, according to Machiavelli, expansion forces a mixed regime to face the options of total destruction or greater longevity. Sparta avoids the former only by forsaking the latter, yet Machiavelli intimates that this is not the most reliable example to follow in order to ensure longevity.

Domestic discord means greater susceptibility to *accidenti* internally, just as expansion means greater susceptibility to them from without. This, of course, entails danger for the regime. But, as we have seen, it also implies the possibility of the regime's still greater enhancement. Republican Rome faced *accidenti* at its inception—it was born of them—but in incorporating them into the regime itself, it became stronger. The staid equilibrium that Polybius ascribes to Spartan domestic politics (1979, 310–11) is not nearly so suitable to political phenomena, conceived by Machiavelli, as is Rome's dynamic domestic equilibrium. Hence, ironically, Sparta is not as stable as Rome! Similarly, regarding foreign affairs, by following an imperial policy, Rome threw itself even more into the face of the unpredictable and became stronger still. Sparta, by choosing not to expand, hoped to exempt itself from the realm of chance and presumed that the necessities of the changing world would not call upon it to do so. To use the metaphor from chapter 25 of *Il Principe*, if Fortune is a woman who favors the active youth, Sparta sought not her favors and made no attempt even to court her, while Rome "struck her down" and "beat her" in the way of Machiavelli's notorious misogynist fantasy. Sparta was lucky to get by for eight hundred years without succumbing to some *accidente* that would bring it down. Rome thrust itself into the realm of *accidenti* and thrived. The conception of "stability" is hereby radically redefined by Machiavelli as something that is ensured by energy and movement, rather than harmony, in both the domestic and external realms.

So far, we have seen Machiavelli's theory of the mixed regime: how it marries the institutional construction of Polybius with the socioeconomic construction of Aristotle and how it is formulated to conform with the conception of political phenomena I set forth as Machiavelli's earlier (a conception that centers upon *accidenti*). The evidence seems to suggest that in a philologically exact sense, Machiavelli formulated his theory of the mixed regime with *accidenti* in mind.

There are other reasons related to stability for which Machiavelli favors the mixed regime. Hans Baron points out the advantages the mixed regime holds over princely rule in Machiavelli's estimation. Republics are superior, in Baron's interpretation, because they better handle emergencies due to the "variety of talents required," which are available to mixed regimes and not monarchies (1961, 223). And indeed, Machiavelli writes that in a republic there is a better chance of having successive virtuous rulers because of elections, unlike the kingdom of Macedon, for instance, which was lucky enough to have only two such rulers in a row (1.20). Relatedly, for Baron, a republic, unlike a monarchy, can more likely produce "a long succession of outstanding men needed to reform a degenerate people" (1961, 223). But this insight, as well as the earlier discussions regarding both the nature of political phenomena and mixed regimes raises further questions. For example, granted the mixed regime is the answer of Polybius and Aristotle to the problems of political stability and longevity and the Roman mixed regime as it relates to *accidenti* is Machiavelli's improved answer to these problems, just *how long* can such a regime last, even with all the advantages it holds over other types of regimes?

And it is curious that Baron, who maintains as his basic premise the incompatibility of *The Prince* and the *Discourses*—one-man rule versus republican rule—should bring up the question of what role "outstanding men" would have in preserving the republic. Attention to this role, in fact, leads to the refutation of his thesis. If we examine the question of *how long* a mixed regime can last in Machiavelli's *Discorsi*, we are compelled to consider his answer—one-person rule at various possible times in the life of the republic. As we shall see, the specter of *accidenti* looms over this subject just as it has over the others.

DEFENDER OF THE CONSTITUTION: DICTATOR OR WISE LEGISLATOR?

Machiavelli is less sanguine at some points than at others about a mixed regime's ability to last. At times, he even seems to question the very project of the *Discorsi*: "Because one cannot give a certain remedy for like disorders that arise in republics, it follows that it is impossible to order a perpetual republic, because through a thousand unexpected ways its ruin is caused" (3.17). To expect a regime to last forever would certainly be to ask for too much; even Rome eventually collapsed. But the search for "remedies" to dangerous "disorders"—for a way to foresee the "thousand unexpected ways" through which a republic is destroyed—is precisely one of the central tasks of the *Discorsi*. Indeed, Machiavelli is generally more hopeful than this passage would indicate.

Although he opens book 3 with the statement, "It is a very true thing that all worldly things have a limit to their life," Machiavelli goes on to explain the essential preconditions for ensuring a long life to things such as mixed regimes. All things pass away but "generally they go the whole course that is ordered for them by heaven, so that they do not disorder their body but keep it ordered in such a mode that either it does not change, or, if it changes, it is for safety and not to its harm . . . I am speaking of mixed bodies, as are republics" (3.1). In other words, a mixed regime may live out a healthy life if it does not change or if the changes it undergoes improve it. We know from the analysis of political phenomena in Machiavelli that it is very unlikely that a political entity can remain free of change. A time will certainly come when it is confronted by some *accidente* to which it must either react and hence change to some extent or perish and thereby (to put it mildly) change for the worst. We also know from the analysis of the foundings of mixed constitutions that a regime that manages to remain relatively unchanged, like Sparta, misses the opportunity to change for the better, as Rome did.

The changes that improved Rome occurred at the city's beginning, and Machiavelli writes that for a mixed regime already underway to maintain itself, it must in some way return to that beginning:

> Those are better ordered and have longer life which by means of their orders can often be renewed or indeed that through some accident outside the said order come to said renewal. And it is a thing clearer than light that these bodies do not last when they do not renew themselves. The mode of renewing them is, as was said, to lead them back toward their beginnings. For all the beginnings of sects, republics and kingdoms must have some goodness in them by means of which they may get back their first reputation and their first increase. And because in the process of time that goodness is corrupted, unless something intervenes to lead it back to the mark, it kills that body of necessity. (3.1)

The causes of this renewal or renovative "return to beginnings" are referred to by Machiavelli first as either an "external accident or intrinsic prudence" (3.1). Later in the chapter, however, Machiavelli refers to the occasions for renewal as either external or internal *accidenti* (ibid.), thus again putting them at the center of the proceedings. An example of an external *accidente* that inspires renewal is the invasion of Rome by the Gauls. That invasion caused Rome to reorganize itself (ibid.). Internal *accidenti*, on the other hand, engender renewal through "law" or the actions of "a good man." Machiavelli declares that it is good if such *accidenti* force the framing of new laws or the action of a single man every 10 years to stave off corruption (ibid.). I shall focus upon the internal *accidenti* inspiring such means of renewal, rather than the external ones with which we are by now so familiar. The tension between the two possible responses to internal *accidenti*—law and personal rule—is one of the most confounding and intriguing aspects of Machiavelli's theory of preserving a mixed regime.

It is not surprising to find an emphasis on the role of "one man" in *Il Principe*: "In affairs of state, . . . when one recognizes from afar the evils that arise in a state (*which is not given but to one who is prudent*), they are soon healed; but when they are left to grow because they were not recognized, to the point that everyone recognizes them there is no longer any remedy for them" (chap. 3, my emphasis). But the role of one man is also an integral part (Baron's discomfort and Pocock's neglect notwithstanding) of Machiavelli's theory of mixed government in the *Discorsi*:

> It is of necessity . . . that in a great city accidents arise every day that have need of a physician, and according as they are important, one must find a wiser physician. And if like accidents ever arose in any city they arose in Rome both strange and unhoped for . . . And although these diseases produce bad effects in a republic, they are not fatal because there is almost always time to correct them . . . [But] if they are not corrected by *a prudent individual*, [they] ruin a city. (3.49, my emphasis)

In both passages, drawn from two different works, we find the concern with political phenomena that I have presented earlier: *accidenti* may be dealt with only if seen from afar and responded to properly and at an appropriate time. The new element here, from the point of view of this study, is the role of the "one who is prudent," the "wise physician," the "prudent individual." Throughout the *Discorsi*, Machiavelli emphasizes the role that one person may need to play in the maintenance of a republic. In 1.9, he writes that one person is necessary not only to found a regime but also to revive it. This claim is reiterated in chapter 55, where he advises that a "kingly hand" is necessary to remodel a republic. In chapter 17, he declares that the goodness of one man can save a regime from corruption. Yet this emphasis on the role of "one individual" is not necessarily as ominous as it sounds. After all, such observations even appear in Aristotle: "Ordinary men cannot see the beginning of troubles ahead; it requires the genuine statesman" (*Politics* 5.8.9).

If a regime must be renewed to maintain itself and if this renewal can take place within the regime through law or through the action of one man, an obvious question poses itself: Are the actions of this one man to be confined or governed by law, or are they to be extralegal? Is the "one individual" supposed to defend the constitution by constitutional or extraconstitutional means? Machiavelli's answer, seemingly, is both.

Machiavelli promotes the institution of the dictator as a legal and constitutional means of dealing with *accidenti* and preserving a mixed regime: "Those republics which in urgent perils do not take refuge either in the dictator or in similar authorities will always come to ruin in grave accidents" (1.34).[24] For Machiavelli, a dictatorship is an alternative to extralegal methods for dealing with *accidenti*. For if a republic has no constitutionally recognized way of establishing a temporary dictator, it faces the choice of adhering to the constitution as it is and perishing or violating it and surviving. But if a mixed regime survives through extraconstitutional means, a bad precedent is set, and in the future the law may not be broken with such good intentions in mind (1.34).

Consistent with this interpretation of *accidenti*, Machiavelli says, with dictatorship in mind, "A republic will never be perfect unless it has provided for everything with its laws and has established a remedy for every accident and given a mode to govern it" (1.34).

Basing his theory of dictatorship on the Roman republican model, Machiavelli defines it as: "giv[ing] power to one man who could decide without any consultation, and execute his decisions without any appeal. This remedy, as it was useful then and was the cause that [Rome] overcame the impending perils, so it was always most useful in all the [accidents] which, in the expansion of their empire arose at any time against the republic" (1.33). A dictatorship was limited by time and task. It was prescribed for a specific length of time, during which the constitution could not be amended. The dictator could suspend laws but not change them (1.34). Machiavelli notes how the Roman dictator could deprive a person of his office but never ban the office (1.35). The decemvirate were bad dictators, according to Machiavelli, because they made constitutional changes (ibid.).

Yet not *all* of Machiavelli's references to the importance of "one individual" in preserving a mixed regime mention the need to confine this crucial figure by the "rule of law" (to use that phrase somewhat anachronistically). The emphasis on constitutional limitations, found in his chapters on dictatorship, is lacking elsewhere. In 1.18, Machiavelli describes how laws—the other internal device of renewal to ward off *accidenti*, besides one-man rule—cannot combat the dangers that arise in a severely corrupt regime. If new laws to check corruption are legislated through corrupt institutions, they will be ineffectual. Corrupt institutions must then be renovated either little by little before corruption is widely known or all at once when noticed by all. Both solutions, according to Machiavelli, are almost impossible. To replace them little by little requires a "prudent man" who can see the corruption from a great distance. Such a man might not exist at the time, and even if he did he would have to convince others of the need for renovation, which would be difficult. On the other hand, to make corrections all at once, the law must be broken, resort must be taken to violence and arms, or (possibly) a prince must be enthroned. A good man would not want to resort to these measures or become such a prince; the bad man would but would not relinquish power and restore the republic once his job was completed (1.18).

Baron misreads this chapter. He believes that Machiavelli has ruled out the possibility of renewal through the means described. For Baron, the chapter "repudiates a program of salvation through the ruthless actions of a princely usurper in the manner recommended in the *Prince*" (1961, 233). But Machiavelli merely demonstrates the difficulty of such reform; he does not "repudiate" its possibility. Machiavelli says only that it is *almost* impossible to renew a regime in this way, and his subsequent discussion of the "kingly hand" of the reformer implies that a "good man" might indeed undertake the violent measures necessary to preserve the regime in the face of emergencies. Further, if one raises the question of the "ruthless" manner of the *Prince*, one should not ignore the potential ruthlessness of the *Discorsi*. In the passage about the "return to beginnings," Machiavelli defines the renewal that will accomplish this in terms of "terror" and "fear" (3.1). Such elements are by no means wholly remote from Machiavelli's theory of the mixed regime.[25] But more importantly, there is seemingly no reconciliation of this tension between law and personal rule—this clash between incipient theories of *Rechtsstaat* and *arcana rei publicae*—concerning constitutionally bound dictatorship, extraconstitutional "prudence," and *accidenti* that the mixed regime, as such, cannot endure.

So how does Machiavelli view the maintenance of the mixed regime in exceptional circumstances? Because *accidenti* are not always of the variety that may be combated or coopted in the Roman manner and often strike a mixed regime that has at least to some extent been corrupted, a dictator may be necessary. But because a dictator's role must be prescribed and the nature of an *accidente* may not, a mixed regime may need to have recourse to a person with even more power than a dictator and with even fewer restrictions restraining him.

We need not draw from this outcome conclusions as sinister as those intimated by Leo Strauss. But neither should this side of the *Discorsi* be ignored, as it often is by such other eminent Machiavelli scholars as Baron, Pocock, and Skinner. It would be illegitimate to conclude that Machiavelli's ultimate and primary answer to the problem of *accidenti* and political stability is something akin to an extraconstitutional tyrant. To do such would mean falling into the either/or dilemma associated with Carl Schmitt: the unrestricted nature of political phenomena necessitates an unrestricted political actor. For Machiavelli, the subterfuge and violence to which a "prudent man" must sometimes resort outside of constitutional prescriptions in order to preserve the regime is only one alternative among many. The variability of political nature for Machiavelli necessitates various responses by which to deal with it. The complexities of the mixed regime itself are such that it may absorb many of the kinds of *accidenti* that the political realm will thrust upon it. Those that cannot may be dealt with by the institution of a dictator. Only as a last alternative does Machiavelli introduce the extraconstitutional "prudent man."[26]

CONCLUSION

I have traced the use of the word *accidenti* through Machiavelli's *Discorsi* in the hope of bringing to light, perhaps slightly more clearly than has been done in the past, Machiavelli's conception of political phenomena. Just as Carl Schmitt, supposedly at the end of modernity, focused upon what was generally considered peripheral, or at least subordinate, to the realm of politics—the exception—and thrust it, with stunning effect, to the center of this realm, so too did Machiavelli, at modernity's supposed beginning, focus upon what was by definition nonessential—the *accidente*. By his consistent use of it, Machiavelli established the contingent or accidental as the central concern of politics. The most important implication of his approach is that one need not adhere to authoritarian or neoabsolutist positions (as did Schmitt) to demonstrate sensitivity to the political exception. On the contrary, support for the principles of moderate or mixed government may itself express this sensitivity. As I noted, Machiavelli suggests that extralegal individual efforts may be necessary to save a regime. But the thrust of his overall presentation in the *Discorsi* is that this is not the only or even the primary means of doing so. Machiavelli willingly devotes the bulk of this work to describing *accidenti* and the more mundane and legal ways in which regimes may deal with them; brilliantly shows how a complex, mixed regime can better adapt to such *accidenti* institutionally and socioeconomically than a unitary regime; and forcefully advocates a constitutionally prescribed dictatorship. As a consequence, he strongly emphasizes the legal over the extra- or supralegal means of defending a regime.

If Strauss is correct that for Machiavelli, contingency is the "enemy," and chance something to be "conquered" (1953, 178; 1958, 167), then Machiavelli could be seen as accomplice in the crime that Schmitt associates with the modern, particularly liberal project—the circumscription or even elimination of the concept of the exception. But while Machiavelli concentrates on the exceptional—the "accidental"—in the hopes of providing for its better management, there is an awe, a respect for the magnitude and irrepressibility of *accidenti* that would fade in those who followed in the Florentine's footsteps. These denigrators of the accidental are the ones whom Schmitt and Strauss are perhaps justified in criticizing.

Notes

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1. The most striking expression of such discontent with liberalism occurs in the recently revived Weimar work of the German constitutional-political theorist, Carl Schmitt. It is Schmitt who declared, in quite dramatic fashion, that the possibility of "the exception" and the likelihood of its management were issues absolutely central to the conduct and study of politics (1988, 5). It is perhaps not an unrelated fact that Schmitt eventually endorsed the Third Reich in 1933.

2. Hanna Pitkin (1984) provides the most comprehensive treatment of *fortuna* itself; Leo Strauss (1958) has grappled most explicitly with Machiavelli's other expressions of contingency, such as chance (*caso*) and accidents (*accidenti*). As will become clear, while I do not support Strauss's strategy of grouping these words into a particular scheme that supposedly reflects Machiavelli's "intention," I do wish to focus upon one of these other expressions of contingency, the *accidente*, to help broaden the scope of scholarly views of Machiavelli's conception of political phenomena.

3. For accounts of the significance of these schools during the Renaissance, see Skinner 1978, esp. vol. 1, chap. 3 and pp. 144–151.

4. For instance, Francesco Guicciardini frequently distinguishes between that which is "natural" and that which is "accidental": "Everything, either by nature or by accident [*per accidente*], ends at some time"; again, "Let no one trust so much in native intelligence [*prudenza naturale*] that he believes it to be sufficient without . . . experience [*l'accidentale della esperienza*]" (1965, 43, 89; 1975, 17, 221).

5. In a certain sense, Machiavelli's "rehabilitation" of the accidental is the obvious precursor of Carl Schmitt's more infamous project: the Florentine had already made that which is *not* the normal case, the exception, the very heart of political matter. Yet there are two important differences, besides the obvious and important historical one, between these two "exceptional" thinkers. First, Schmitt was convinced that any form of mixed regime—a government where sovereignty is to some extent divided—could not hope to survive the challenge of an exception. Second, Schmitt was silent regarding what form an exception might take and was reluctant to provide historical examples of "exceptions" from the past. Schmitt wrote, "The exception . . . can at best be characterized as a case of extreme peril, a danger to the existence of a state . . . But it cannot be circumscribed factually" (1988, 6).

6. English renderings from the *Discorsi* have been generously provided by Harvey C. Mansfield and Nathan Tarcov from a recent draft of their new translation, soon to be published by the University of Chicago Press. The Italian references are from the Rizzoli edition of 1984. Book and

chapter numbers will appear in parentheses following the quote or reference.

7. All English references to *Il Principe* are to the Mansfield edition (1985) and all Italian quotations are from the Rizzoli edition (1950). Again, although the advice given here relates to principalities and not mixed regimes, I believe the passage is still pertinent to my concerns here. I would extend Skinner's claim that "the underlying political morality of the two books [*The Prince* and the *Discourses*] is the same" (1978, 183) to maintain that the underlying *understanding of political phenomena* in the two books is the same. In any case, Machiavelli is referring to the foreign policy of the Roman *republic* in this passage.

8. Cf. *Discorsi* 1.32.

9. Some of Machiavelli's arguments are less convincing than others concerning how to foresee and then deal with an approaching *accidente*. In the chapter "Before Great Accidents Occur in a City or a Province, Signs Come That Forecast Them or Men Who Preach Them," Machiavelli claims, "No grave accident in a city or in a province ever comes unless it has been foretold either by diviners or by revelations or by prodigies or by other heavenly signs" (1.56). Machiavelli goes on to cite how "everyone knows" that Savonarola predicted the invasion of the French, how the same occurrence was preceded by visions in the sky, how Lorenzo's death was preordained by a lightning bolt that struck the highest point of the cathedral, and how Soderini's downfall was also symbolized in advance by lightning. How seriously are such examples to be taken, presented as they are by such a renowned antireligious theorist as Machiavelli? What are we to make of such a passage written by one who elsewhere has shown no regard for piety except its political usefulness and who praised the ancient Roman fraudulent use of auspices (1.14)? And even if Machiavelli is indeed sincere in his declaration, how helpful are such instances for really foreseeing political phenomena? Leo Strauss interprets Machiavelli's suggestion that signs may precede *accidenti* to be part of the Florentine's overall plan to subvert the traditional understanding of the divine and of nature, reducing both ultimately to the element of chance. Strauss reinforces this interpretation by linking these "foretold" *accidenti* to those that in another chapter, Machiavelli describes as being manipulated by Roman military leaders (3.14). Strauss's aim is to persuade his reader that for Machiavelli *accidenti* are phenomena that not only happen *to* a political entity but also can be consciously created and manipulated *by* a political entity. This second example proves, according to Strauss, that Machiavelli did not believe that the omens or signs that preceded the *accidenti* cited in the previous example were anything of the sort but were rather occasions for manipulation by political agents who knew how to put chance occurrences to their own use (1958, 210–23). In a recently published work challenging Strauss's interpretation on several grounds, Anthony Parel suggests that Machiavelli invokes these signs that precede *accidenti* in good faith (1992, 40). Parel bases this claim on his overall thesis that "a pre-modern cosmology and a pre-modern anthropology underlie Machiavelli's political theory" (p. 153). He identifies this cosmology–anthropology as "Ptolemaic-astrological" (p. 67) and insists that this is reflected in the passage in question. According to Parel, Machiavelli's belief in astrology or a cosmological order that is governed by celestial bodies would allow for the relationship between great events and sudden changes in the heavens from which predictions could be made. While one might not wish to attest to the efficacy of all the evidence in Strauss's "subversion" thesis, it might certainly be argued against Parel (as I do here indirectly) that Machiavelli is attempting to purge political discourse of just those conceptions of political phenomena that posit some sort of order, be that order Christian, Aristotelian, or even astrological.

10. Other instances where Machiavelli discusses events in terms of "chain reactions" but not necessarily *accidenti* appear in 1.7–8, 11. It is interesting that Machiavelli's universe, which is characterized in general by disorder, has within it pockets of such rigid causality.

11. Cf. the famous passage in chapter 25 of *Il Principe*, where Machiavelli advises audacity in action over caution on the grounds that "Fortune is a woman" and favors those who behave in daring fashion.

12. According to Kurt von Fritz, Polybius' theory is one of the most determinist of the mixed regime. As he explains, even for Aristotle, unlike Polybius, there is no necessity of degeneration from one specific kind of regime to another specific kind in any cycle: an unstable regime of any sort may degenerate into one of any other. Polybius, who probably was not familiar with Aristotle, particularly not the *Politics*, probably owed such rigidity to a particular reading of Herodotus or a faulty reading of Plato (Fritz 1954, 67, 49, 74, 67). Machiavelli's use of *accidenti*, in this instance, is much closer to his use of the term *umori* than to the term that is my focus here. For an intelligent discussion of *umori* in Machiavelli, see Parel 1992, chap. 6.

13. In his criticisms of Strauss's interpretation of Machiavelli, Parel cautions against overlooking "the cosmological roots of [Machiavelli's] political *theology*" as they are manifested in astrology and collapsing (as he claims Strauss does) Machiavelli's distinct notions of nature and chance: "Strauss makes the Machiavellian notion of chance look like the only causal 'agent' operating in his (Machiavelli's) universe, and he makes it look like an immanent cause at that" (1992, 52, 85; my emphasis). But this is precisely the point. Strauss, perhaps because of the influence of Carl Schmitt on his thought, is able to recognize that Machiavelli is ordering the universe—he is founding it—on the basis of the absence of all order. The immanent cause is *no* cause. For Machiavelli, as perhaps for Schmitt, there is nothing (except the monumentally important element of human action) beside chance—exemplified here by the *accidente*. This is the foundation of Machiavelli's cosmology, his metaphysics; and this is why it is an anticosmology, an antimetaphysics. Thus Strauss is right to emphasize political *theology*, or what he calls Machiavelli's "quasi-theology," over an astrological cosmology, for Machiavelli is nothing if not a political *theologian* in the Schmittian sense of the word: "By substituting 'accidents' for 'chance', [Machiavelli] deliberately blurs the distinction between nature and chance in order to indicate the common origin of both belief in gods and knowledge of nature" (1958, 215, 218–19). Just as Machiavelli, whom Strauss elsewhere describes as a philosopher of "extreme situations," and of "the extreme case" (1953, 162, 179), subverts the traditional notions of the divine and nature in a work, the *Discorsi*, on how best to deal with political phenomena, Schmitt, as Strauss well knew, set forth his theory of political phenomena and the exception in a work specifically titled *Political Theology*. This is not to suggest that Strauss's interpretation of contingency in Machiavelli is incontestable. Parel is justified in expressing discomfort with what might be called Strauss's "hierarchy of subversion" interpretation of Machiavelli, wherein the Florentine supposedly reduces God to nature, nature to fortune, and fortune to accidents (1992, 84; cf. Strauss 1958, 216). While accepting Strauss's notion that Machiavelli attempts to supplant order-based notions of political phenomena previously grounded in religion or natural law, I do not think Machiavelli's attempt follows the coherent and seemingly air-tight strategy that Strauss sets forth. I maintain that *accidenti* are not necessarily reductions from fortune and thereby subversions of it but are rather fellow phenomenological manifestations of chance or contingency and perhaps more useful manifestations for students of political science to examine.

14. Aristotle also concerned himself with how justice was intertwined with stability, particularly in book 4 of the *Politics*.

15. All English translations are from the Barker edition of 1958.

16. Cf. Fritz 1954; Walbank 1972.

17. In brief, in small communities that have survived catastrophes, the strongest or bravest becomes leader and monarchy is instituted, and with the expansion of families and social relationships, the idea of kingship is born along with the notions of goodness and justice and their opposites. These ideals, rather than force, subsequently become the

support of his rule. But the honors and abundance that accrue to this person and his heirs cause abuses and, soon, tyranny. The best rise up against it to set up an aristocracy, whose heirs also abuse their positions to become oligarchs. These are overthrown by a democracy, which in turn is corrupted into mob rule (Polybius 1979, 305–9).

18. This difference was pointed out almost simultaneously by Gennaro Sasso (1958, 306–15) and Leo Strauss (1958, 222).

19. Harvey Mansfield makes this observation, with credit to Nathan Tarcov (Mansfield 1979, 38).

20. Consider one other such problem. Pocock severely underestimates the role of the "legislator" in the *Discorsi*. It is true that Machiavelli downplays the role of foresight that Polybius associates with Lycurgus the legislator, but as I shall show, both foresight and the figure of the legislator become important for Machiavelli when he deals with the *maintenance* of a mixed regime rather than its *founding*.

21. My treatment of Machiavelli's relationship to Aristotle's *Politics*, particularly his incorporation of the Aristotelian socioeconomic theory of the mixed regime, owes much to Pasquale Pasquino and his work in progress on Machiavelli, Hobbes, and the origins of modern political thought, *Politics and Threat*.

22. For several essays related to republican discord, see Skinner, Bock, and Viroli 1990.

23. Machiavelli apparently disregards Aristotle's classification of Sparta as an imperial and expansive regime in book 8 of the *Politics*.

24. Cf. *Discorsi* 1.33, para. 1.

25. For the most arresting treatment of renovation and terror in the *Discorsi*, see Strauss 1958, esp. 167.

26. In his work of the early 1920s on dictatorship, Schmitt (1921), who proved to be a very careful reader of the *Discorsi*, expressly advocates Machiavelli's republican-based theory of temporary dictatorship over less constitutionally restricted ones in order to manage emergencies. But with his subsequent "fascination" with the exception, Schmitt abandoned the former for the latter, extraconstitutional type of dictatorship, because the sheer mystery and magnitude of the exception, according to Schmitt, defied *any* systematic treatment. Machiavelli, as we have seen, is able to maintain a broad and diverse theory of exceptions. He does not lump all *accidenti* together under an unanalyzable concept, such as *Ausnahmezustand*. As a result, he does not need to resort solely to extraconstitutional alternatives to manage the exception. Because Schmitt never discusses, describes, or suggests just what an exception might look like, his supralegal sovereign appears to be the only logical alternative in emergency situations—which may indeed have been his full intention.

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UTILITY, PROPERTY, AND POLITICAL PARTICIPATION: JAMES MILL ON DEMOCRATIC REFORM

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On the philosophical plane, James Mill's political thinking began from a model of man quintessentially utilitarian in constitution. Starting with individual agents, it was to his account of the science of human nature that he turned in the quest for a science of politics suitable for the modern world. If James Mill's science of politics was individualist in character, it was neither automatically nor necessarily democratic in the practical political arena. On that subject, everything turned on the question of judging when (or if) individual capacity had reached an acceptable standard. This criterion proved to be sufficiently malleable to allow him to appear either expansive and democratic or narrow and elitist, as the case required.

While it would be something of an exaggeration to say that a great controversy rages over James Mill's role in the movement for democratic reform in Britain during the first three decades of the nineteenth century, it is certainly true that sharp divisions have existed over exactly how that role is to be cast.[1] According to some, Mill was the spokesman for the political demands of the middle classes, the advocate of a reform of parliament designed to make it representative of the interests of private property.[2] According to others, Mill was more the democrat he often declared himself to be, the advocate of a reform of parliament designed to make it more broadly representative of "the people."[3] Doubtless these divisions persist because so much is at stake. The democratic commitment of those varieties of liberalism that find their basis in utilitarian theory is ultimately in question. Perhaps this is why today as in the past one thing, at least, that seems to be agreed on among most of the commentators on James Mill's politics is that we cannot have it both ways and that we have to locate James Mill's politics squarely in one or other of these putatively opposing encampments.

The possibility that Mill's politics might have embraced both of these opinions has, of course, not been entirely overlooked. But those who have traveled this interpretive path have almost uniformly reached the conclusion that Mill was inconsistent, confused, or both.[4] The appeal of this line of interpretation is that it at least allows one to avoid the intellectual gymnastics that seem so common in those readings which would have us paint Mill's politics in either black or white. For on this alternative reading, one can admit all of what Mill actually had to say on the subject of politics, rather than having to find reasons why one might legitimately put to one side those remarks which do not blend neatly into one's particular representation.

We wish to suggest another interpretive strategy, namely, that James Mill's contributions to the science of politics—"that master science," as he once called it (1818a, 708)—might be more satisfactorily understood if due account were taken of the fact that in unfolding his vision of a reformed polity, Mill found it necessary to provide a mapping from the principles on which representation was made to rest, to the actual conditions of the societies in which those principles were to be applied. The "philosophical Mill" and the "polemical Mill" appear as characters in this drama in essentially the same costumes they have worn in previous renderings of Mill's politics. The difference, however, is that on the reading we propose it is necessary neither to elevate the one over the other nor to see them as appearing as representations of some basic or underlying inconsistency of thought. In what follows, we shall attempt to document how, beginning and ending with a framework of concepts in which utility and knowledge were the desiderata for political participation, Mill was led (for reasons both theoretical and practical) to associate property with the requisite capacity for political judgment.

THE PRINCIPLES OF REPRESENTATION

When James Mill opened the entry on "Government" for Macvey Napier's Supplement to the *Encyclopedia Britannica* with the assertion that "the whole science of human nature must be explored, to lay a foundation for the science of politics" (1820a, 3), it was, for him, no recently formed opinion. In a review of Gaetano Filangieri's *Science of Legislation* for the *Literary Journal* as far back as September 1806, Mill had already revealed his preference for a maxim he there attributed to Filangieri, namely, "Political laws are ultimately founded on certain fixed principles, derived from the nature of man, . . . [and] according as laws coincide with these principles they are good, according as they discord with them they are bad" (1806c, 228). Whether or not the "fixed principles" concerning the "nature of man" to which Mill subscribed in 1806 were derived from utilitarian philosophy or from the Scottish moral-sense tradition of Reid and Dugald Stewart,[6] it is clear that by the time of the essay on "Government," the principles of representative government, according to Mill's plan for a new constitution, were to be derived from nothing less than the application of the utilitarian theory of choice and action to politics. Mill spared his readers the journey into the details of that theory in the essay on "Government" itself, remarking only that "to understand what is included in the happiness of the greatest number, we must understand what is included in the happiness of the individuals of whom it is composed" (1820a, 3) and that "the lot of every human being is determined by his pains and pleasures; and that his happiness corresponds with the degree in which his pleasures are great, and his pains are small" (1820a, 4).

Mill's rendition of the utilitarian theory of human nature began with a model of man as an aggressive and domineering pleasure seeker, who, if unchecked, would "as a law of human nature . . . take from others anything which they have and he desires" (1820a, 8). The same figure was implicated in the essay on "Jurisprudence": "As every man desires to have for himself as many good things as possible, and as there is not a sufficiency of good things for all, the strong if left to themselves, would take from the weak every thing, or at least as much as they pleased" (1820b, 4). In this way, it was on the presumption that "there is no limit to the number of men whose actions we desire to have conformable to our will" (1820a, 11) that Mill grounded his arguments for both the actual need for, and the necessary structure of, government. Inevitably, this led him to a conception of civil government in which the state was, at once, a human institution rooted in the individual's need for protection and potential threat to that very security (1820a, 15). Utility thus explained the origin of government and utility mandated that its powers over individuals should be kept limited and small (see 1825b, 196).

According to Mill, all the "difficult questions of Government relate to the means of restraining those, in whose hands are lodged the powers necessary for the protection of all, from making bad use of it. Whatever would be the temptations under which individuals would lie, if there was no Government, to take the objects of desire from others weaker than themselves, under the same temptations the members of the Government lie, to take the objects of desire from the members of the community, if they are not prevented from doing so" (1820a, 5). This conception of a political world in which power must constantly check power led Mill to reject in principle the three "pure forms" of government—democracy (the many), aristocracy (the few), and monarchy (the one)—as well as any theory of a mixed or "balanced" government of all three (see 1825b, 197).[7] No set of powers so aggressively construed could possibly be expected to compromise:

> The Democracy or the Community have all possible motives to endeavour to prevent the Monarchy and Aristocracy from exercising power, or obtaining the wealth of the community, for their own advantage: The Monarchy and Aristocracy have all possible motives for endeavouring to obtain unlimited power over the persons and property of the community: The consequence is inevitable; they have all possible motives for combining to obtain that power, and unless the people have power enough to be a match for both, they have no protection" (1820a, 16).

It was on the basis of this demonstration—and the corollary that any interest not congruent with that of the community was "sinister"—that Mill constructed his theory of representative government. In achieving representative government, the "real object to be aimed at in the composition of the legislature," Mill argued, was "to prevent the predominence of the interest of any individual or of any class; because if such interest predominates, . . . it will be promoted at the expense of the community" (1826d, 781; see also Hamburger 1963, 22). The only form of government that would check the tendency of the monarchy or the aristocracy to domineer was one that placed the power of government (identified as the legitimate power to check) in the community (see 1824a, 211, 219; 1825b, 201). However, the people themselves (democracy) could not be entrusted actually to exercise these powers, therefore they must be handed over to a "body of men, called Representatives" (1820a, 18). On the grounds that the entire community would be so large as to be "essentially incapable of business" and that in large assemblies, "so many persons desire to speak, and feelings, by mutual inflammation, become so violent, that calm and effectual deliberation is impossible" (1820a, 6), Mill was led directly to his advocacy of representative government. The question remained, however, as to how these representatives themselves were to be prevented from domineering: "*Quis custodiet ipsos custodes*?" (1835d, 15). Mill's answer was twofold. On the one hand, short but unspecified terms of office would, he claimed, ensure that representatives identified their own interests with those of the community (1820a, 18). On the other hand, Mill argued that it was necessary to limit and shape the character of the *electors* eligible to choose such representatives to those whose interest could also be ascertained to be identical to that of the community. The mechanisms for this second and more formative political task were the secret ballot and education.

In the *Parliamentary Review* for the sessions of 1826–27 and 1827–28, Mill contributed an article arguing that the prevailing system of open voting promoted the corruption of politics in at least two ways. First, it obviously enhanced the ability of the wealthy to give a veneer of electoral legitimacy to their own sinister interests by exerting pressure on those beneath them to cast votes in their support: "If the poor man cannot conceal from the rich man how he votes, the rich man knows to a certainty when his price commands his commodity, and he can make sure of it" (1827, 365). Second, the open voting system was necessarily morally corruptive of the poor, since "the voter, whether he be a good man, or a bad man, would, if he followed his own inclination, vote differently from the mode in which influence is exerted to induce him to vote." The consequence for the citizen, Mill argued, was that if "he is a bad man, and disregards a promise, he votes as he pleases, knowing he may do so with impunity; and his promise passes for nothing. If on the contrary we suppose that he is a good man, the good man knows that it is a bad thing to make a bad promise; but a worse thing to keep it" (ibid.).[8] Either way, the virtue of the would-be voter was impuned.

In Mill's hands, the secret ballot became the single most important weapon against rule by the wealthy: "The ballot, and that alone, can enable [the people] to choose, and render the British constitution in reality what it now is only in pretence" (1830, 13). Elsewhere, with greater rhetorical flourish, Mill wrote: "Make voting secret, and who will pay for a vote which is of no value, when it cannot be known? Make voting secret, and who will incur the expense of bringing distant voters to the poll, who may all vote for the opposite party?" (1827, 362; see also 1825b, 201–2). Mill continued to place faith in the secret ballot, even as late as 1835, as that element of democratic reform most necessary to secure good government.[9] On a theoretical level this is not surprising, since the secret ballot goes well with the fear of corrupted interests so strongly expressed in all of Mill's political writings. Without electors whose choices can be made independently there could be no checking of the influence of "sinister interest" in the constitution. Secrecy, Mill believed, would ensure independence (1825b, 228). Unfortunately, the secret ballot alone could not prevent the rise of another, equally sinister interest—that of the poor. Here Mill's answer was not to "check" it but to educate it.

"The virtue of people, you say, is weak. Unhappily it is so, deplorably weak," Mill wrote in the July number of the *Westminster Review* for 1830 in an article on the ballot. However, its improvement required more than franchise or electoral reform; an indispensible component to Mill's parliamentary reform proposals was a plan of education. The details of that plan are of less importance to this discussion than is its unambiguous purpose, namely, to create a community of shared (common) interest out of individuals whose interests could neither naturally nor originally be understood as coincident or bound up with those of others. When one turns to the essay on "Government," the paramount necessity of the existence of such a coherent moral and political "community" in order to make his proposal for a reformed government "representative" through the "identity of interests between ruler and ruled" becomes clear (see also James Mill 1825b, 213). Referred to by Mill as an "exposition of the Elements of Political Knowledge," the essay "Government" isolated middle-class morality as the most important component of political knowledge available to the masses. Consonant with his general and rather sketchily crafted argument in the essay on "Education"—that the foundation of mental life is grounded on associations (mental sequences)—it was necessary for political knowledge and effective political participation that the individual develop the correct associations, or "primary habits," of mind (1818b, 71–99, esp. 93–96; see also Mill 1829). The appropriate habits or associations for the laboring classes were to be formed through their assimilation of the moral norms and values of that "virtuous and intelligent rank." The middle rank, which according to Mill "gives to science, to art and to legislation itself, their most distinguished ornaments, . . . advice and example" (1820a, 32),[10] became for him a vanguard in the formation of those opinions identified with the interests of the community.[11]

In the essay on "Government," these norms provided the foundation of the community and enabled its constituents to identify as representatives those individuals whose interests were consonant with it. Here, the discussion of creating a genuine community of interest that could safely select representatives who conveyed its virtues into politics replaced any direct discussion of the more familiar radical proposals for "universality, equality, annuality and secrecy."[12] Despite Mill's allegience to "the plebian, the democratical" (1830, 6), he would ultimately do little more for the theory of representation in the essay on "Government" than substitute a new virtualism—one based on age, gender, and "identity of interest"—for the old. Within this framework, it was Mill's apparent willingness to view the laboring class as a threat and, at times, to envisage their principal political role as being one that should be exercised *outside* of government—poised and ready to check the sinister interests of "their Rulers" (1820a, 19)—that led Bentham to characterize Mill's democratical commitments as equivocal. According to Bentham, Mill "argues against oppression less because he loves the oppressed many, than because he hates the oppressing few. He fights for the people—[not] that he cares for the suffering people, but that he cannot tolerate the suffering-creating rulers" (Bentham 1838, 10:432).[13] In this way, Mill's characterization of the actual political function of even an expanded electorate remained more reminiscent of an eighteenth-century vision of popular protest.

In shifting the focus of achieving "good government" away from the virtue of the representative and placing it instead on that of the community of electors, Mill's theory of government took, at best, a tentative step towards modern representative democracy. It ensured that his approach to expanding the franchise was almost wholly negative—so much so, that when applied it proved to exclude from the franchise all but the "aggregate males" ("all males of sound mind and mature years," as he put it in his famous attack on the opinions of the Edinburgh Reviewers [1825b, 222]) who could meet the "indentity of interest" criterion (1820a, 21).[14] In contrast to visions of representation that might have placed a value on bringing into government the existing diversity of interests (especially class interests) present in the community, James Mill's theory continued to stress commonality and uniformity. The question Mill had not yet answered in any satisfactory or modern way, however, was exactly who it was among "the people" that made up this community.

Mill's starting point in the consideration of the extent of the franchise once again bears all the hallmarks of utilitarianism. He set out to determine the capacity for political decision making upon the premise that what was required for good political choice was the full knowledge of one's own interest and the wherewithal to act rationally upon it. Mill then resolved the problem that individuals might entertain mistaken views of interest in a typically utilitarian manner: education was the answer. Maintaining that the "evils which arise from mistake are not incurable" on the grounds that "if parties who act contrary to their interest had a proper knowledge of that interest, they would act well," Mill concluded that all that was necessary was knowledge: "Knowledge, on the part of those whose interests are the same as those of the community, would be an adequate remedy"; for Mill, of course, knowledge was "a thing which is capable of being increased" (1820a, 29; see also 1824b, 466 and 1826c, 268). At issue here is both what *type* of knowledge is necessary to politics and the relationship between that knowledge and political stability. As to the type of knowledge required beyond the practical instruction to be derived from observing the operation of the "political machine" (1835d, 23), Mill adverts to the customary utilitarian conception of knowledge as individual mental capacity developed through formal education. On this line of argument, education "is the means which may be employed to render the *mind* . . . an operative cause of happiness" (1818b, 41; emphasis original).

Once more, the utilitarian model of man is placed at the center of Mill's argument. If individuals are seeking to act in ways that promote their own interest (i.e., if utility is their objective), they must possess full information not only of the alternatives available to them but also of the utility they will derive once a particular course of action has been chosen. "Good choice" mandates the full knowledge of possibilities *and* consequences. In the essay "Liberty of the Press," the case was made in unambiguous terms: "The very foundation of a good choice is knowledge. The fuller and more perfect the knowledge, the better the chance . . . of a good choice" (1821, 19). Years later, in the *Analysis of the Phenomena of the Human Mind,* Mill would return again to the same theme: "For the most perfect performance of acts of prudence, the greatest measure of knowledge is required" (1829, 2:282–83).

As to what it was that this type of knowledge had to contribute to political stability, Mill's answer was as consistent as it was unequivocal: educated individuals would come to see their own interests as being in conformity with those of "the community." Knowledge made things safer; its absence threatened political stability. Moreover, if education failed to suffice to bring an individual's interests into conformity with those of the community, discipline would be required. So much so, in fact, that since "there are some minds with which you cannot be sure of being able in every case to bring evidence, as it were, in contact," it would be necessary to arm magistrates "with a coercive power" (1818a, 717).

Having deduced from the principle of utility the proposition that representative democracy was the basic means of securing good government and having shown that "intellectual aptitude"[15] among the voting public was the only prerequisite to the choice of suitable representatives, Mill turned his attention to the question of the property qualification. The argument ran as follows. A high qualification must be rejected because it would yield just another form of aristocratic or oligarchic government. A low qualification too, must be rejected: "[A] very low qualification is of no use, as affording no security for a good choice beyond that which would exist if no pecuniary qualification was required" (1820a, 22), since the votes of the propertyless would be so few as not to disturb the interest of the rest of the "community." Interestingly, however, in the essay on "Government" Mill did not quite conclude that therefore no property qualification was necessary.

Instead, Mill posed a rather different question. If one thought that such a qualification was needed, upon what principle might it be determined? On the argument that it was "not easy to find any satisfactory principle to guide us in our researches" (1820a, 23), Mill acquiesced to the property qualification, but with the proviso that it be fixed at a level that ensured the inclusion of "the majority" of the community. It seemed to Mill "hardly necessary" to "carry the analysis of the pecuniary qualification" any further. That said, two features of this argument should be kept in mind. The first is that Mill still maintained the primary principle of his science of politics, namely, that it was "the interests of the community" that had to be conveyed into politics, not the "interests of property." At the same time, however, Mill conceded that a secondary *association* between the possession of property and the interests of the community might exist. Such an association would allow a subset of the community—the property-owning middle class—to convey the interests of all safely into government. In this sense, property for Mill appears as a proxy for the community interest and not as the dominant vehicle of political exclusion.[16]

This is not to say that Mill did not articulate a principle of exclusion. In casting his philosopher's eye across the list of names of potential electors, Mill constructed an interesting exclusionary criterion. "One thing is pretty clear," he asserted, namely, "that all those whose interests are indisputably included in those of other individuals, may be struck off without inconvenience" (1820a, 21). It was thanks to this particular criterion that women and children (with the age of 40 being Mill's preferred boundary between minority and majority) were struck off Mill's electoral register (ibid., 22).

Several things need to be said about this argument. In the first place (confining ourselves to matters of logic for the moment), Mill's exclusionary rule fits uneasily within the utilitarian calculus. The application of utility theory to any problem of choice requires (among other things) the assumption that agents possess the capacity to choose—that they have full information as to the possibilities and consequences of their actions. Models of rational self-interest, in other words, assume not just rationality but also knowledge. When addressing the problem of who should be excluded from the electoral register, it was therefore logical to check that each of these requirements was met by voters. Thus, in certain parts of the essay on "Government," Mill was led, quite consistently, to advance education as a criterion against which to judge the suitability of electors. However, the associated idea that when one agent's interests were "included" in another's, utility theory mandated the exclusion of one or other of them is false. Under a simple majority-rule voting procedure, in a society that consisted of more than two individuals, even if a woman whose interests were supposedly "included" in those of some other man had *identical* preferences to that man, political choice would in general fail to satisfy Mill's criterion of the greatest aggregate utility.[17]

The logic that inspired Mill's new virtualism comes out particularly clearly in a short correspondence between David Ricardo and Mill on the publication of Mill's *History of British India*, where Mill seems to have applied it to determine the mode of government best suited to that colony. Ricardo expressed his doubts to Mill as to whether "the Government and laws of one state of society" were well "adapted for another state of society" (Ricardo 1951–73, 7:22). This, Ricardo correctly noticed, was "one of the great difficulties of the science" (ibid., 229). His doubts arose from an obvious consideration, namely, that the "people of England, who are governors, have an interest opposed to that of the people of India, who are the governed, in the same manner as the interest of a despotic sovereign is opposed to that of his people" (ibid., 239).

Furthermore, since the British public's apathy toward the administration of India was taken as a given by Ricardo, he held that even the "outside" check of public opinion at home was not likely to be "very active and will therefore not much tend towards the correction of abuses" (ibid.) in colonial administration. He was led, therefore, to wonder whether the "salutory dread of insurrection" was all that remained to check "misrule and oppression" in that country (ibid., 241). Ricardo, however, then proceeded to put his finger squarely on the problem with the new virtualism of Mill:

> Are we to fix our eyes steadily on the end, the happiness of the governed, and pursue it at the expense of those principles which all men are agreed in calling virtuous? If so might not [any] ruler, disregard all the engagements of his predecessors, and by force of arms compel the submission of all the native powers of India if he could show that there was a great probability of adding to the happiness of the people by the introduction of better instruments of government. (Ibid., 241–42)

The difficulty Ricardo foresaw, then, was precisely a problem at the core of Mill's theory of representation. Furthermore, when that theory was grounded in the theory of utility, the problem took the form of having to determine "how to balance one object of utility against another" (ibid., 242). In short, what Ricardo seems to have been raising here was the whole question of interpersonal comparisons of utility (and this, a good 40 years before the younger Mill came across it in the course of his elaboration of the harm principle in *On Liberty*).

No reply from Mill to these queries is extant. But the correspondence between the two surrounding Mill's *History of British India* impinges directly on the questions Ricardo raised, and it aids in clarifying the character of Mill's thinking on these points. In his very first letter praising Mill's performance, the ever modest Ricardo declared that he was now "anxiously disposed to understand" the science of legislation, and that he entertained "sanguine expectations" as to the "practicability of improvements in legislation" (ibid., 228–29). To this, Mill replied that he had no doubt that "we shall now understand one another" on that subject, and he asserted that "the ends are there, in the first place, known—they are clear and definite" and that it remained only "to determine the choice of means" (ibid., 234–35). In saying this, of course, Mill was no more than repeating the method he applied in legitimating representative government in England: it was the necessary means to the greatest happiness. In India, for Mill at least, the means would be different. When Ricardo politely enquired of Mill as to what one should do when exclusive focus on the ends led, as it did in the case of India, to the justification of an imperial despotism, Mill was silent (see, Hutchison, 1953).

It seems clear, however, that if the essay on "Government" is to be taken at its word (recall that it purports to show that *only* representative government is sanctioned by the principle of utility), then the idea that the government of India could safely be entrusted to the members of the Imperial Parliament at Westminster would contradict his best-known contribution to the science of government. If, on the other hand, we seek to rescue him from contradiction by reminding ourselves that the principle of utility sanctions only a government representative of those capable of knowing what is in their own best interest and acting upon it, and that in nations like India this might be a very few (or none), then it would appear that we might have plunged him into a deeper quagmire. For in that case, the idea of representative government as a government of the "Democracy or Community" would seem to be fatally compromised.[18]

Received opinion seems to be that it was left to John Stuart Mill to attempt to deliver the utilitarian theory of government from this impasse (see D. Thompson, 1976, 25–27). The younger Mill freely abandoned the claim that the principle of utility sanctioned only equal representation (even at home), and, without a second thought, declared that the ignorance of savages rendered enlightened imperial rule superior to a "native despotism" (J.S. Mill 1861, 409). But if James Mill was silent on the relation between utility and self-rule in India in his correspondence with Ricardo, he had not always been so reluctant in expressing the implications of his science of politics when it was to be applied to "underdeveloped" political cultures. In an article on Spanish America in the *Edinburgh Review* in 1809, the elder Mill was as clear as his son was to be some 60 years later. Proceeding on the familiar premise that the "ignorance and irritability" of "the people" might "be worked upon by men of evil intentions," Mill held that in the project of establishing the conditions for political liberty in Spanish America, "as much as possible should be done *for* the people,—but nothing *by* them" (1809, 304–5).

In conformity with his well-established criterion of appropriate aptitude, Mill asserted that there was "one danger in rendering the basis of representation too wide" namely, that "you incur the inconveniences of the ignorant and precipitate passions of the vulgar" (1809, 308). Mill referred his readers to Bentham's *Fragment on Government* for further enlightenment on the subject. In Mill's hands, then, the task of settling on a stable equilibrium between the level of knowledge attained by the general public and the extent of representation became (and remained) the central problematic of the utilitarian theory of government. In all of his discussions of the principles of representation, James Mill neither abandoned it, compromised it, nor obscured it. However unacceptable his argument might seem to be today or however comforting it might be to endorse some (but not all) of its aspects, it does violence to the historical record to attribute a contemporary disquiet over such matters to some putative problem with Mill's own contribution to the science of politics.

REPRESENTATION IN PRACTICE

If one considers just how many of "the people" of Britain would have been enfranchised under James Mill's suggested scheme in the essay on "Government," the result turns out to be interesting. Taking the demographic profile for Great Britain and Ireland for 1820, the year Mill's essay appeared, the number of males aged 40 or above was approximately 2.1 million that is, 10% of a total population of about 21 million (see Mitchell 1988, 9–10). Assuming that the then existing electorate numbered approximately five hundred thousand adult males, a little arithmetic yields the conclusion that had all of Mill's 40-year-old men been enfranchised, the scheme would have quadrupled the electoral franchise.

Some appreciation of the adjustment that the practical adoption of Mill's scheme would have entailed can be gauged by recalling that when a bill for parliamentary reform was finally introduced in 1832, Lord John Russell conjectured that if enacted, the measure would add about half a million voters to the electoral rolls of a kingdom of some 24 million inhabitants. Had this estimate been accurate, the First Reform Bill would have roughly doubled the existing electoral franchise. As it turned out, the net addition to the franchise was a little over three hundred thousand (see Halévy, 1913–32, 3:27, n. 5). An actual expansion of the elective franchise on James Mill's scheme would therefore have been seven times greater than that eventually secured under the 1832 Reform Bill.

These exercises, however, reveal only part the distance that separated the Whig and Radical reformers of the day. It has also to be remembered that for most of the Whig reformers the First Reform Bill stood at the end of the road to democracy, while for the radicals it marked only its beginning. Thus, for example, as home secretary in Lord Melbourne's cabinet, Lord John Russell declared at the opening of Parliament after the 1837 general election—in the speech that earned for him in Radical circles, the nickname "Finality Jack" (see Wallas 1925, 366; Walpole 1889, 1:289–90)—that the wide scope of the constitutional readjustment worked by the First Reform Bill was acceded to by his Whig colleagues precisely because it was meant to be final.

As far as James Mill was concerned, the practical reform of the franchise accomplished in 1832, while tending in the right direction, was a mere "crumb of reform" (1835d, 12). In the four years that remained of his life after the First Reform Bill, Mill steadfastly ridiculed the idea that the act of 1832 could be thought of as a final measure. Declaring it to be the work of "half-and-half reformers," Mill continued to play out the central themes of his science of politics against a new backdrop. Nothing had been done to remove the abuses concomitant upon the open voting system (ibid., 14–15; 1835a); the House of Lords remained a seat of aristocratic power with the ability to subvert the wishes of the House of Commons (1836, 297–301); the "injustice and oppression" of sinecures and other forms of political privilege remained in place (ibid., 293–94); the religious establishment remained intact; and even the House of Commons itself still needed to be "chosen by the people, not nominally, as, to a great degree, it is at present, but actually, and in truth" (1835c, 51). In short, the British electoral reform of 1832 provided none of the securities to good government that Mill had long demanded.

Another arena in which Mill's application of his theory of government may be seen in action was found in his reflections on the popular disturbances of the day. Indeed, Mill addressed the events of Peterloo in the essay on "Government" itself. "What signify the irregularities of the mob," he asked, when it was "more than half composed in the greater number of instances, of boys and women?" And then he came to the point:

> What signifies the occasional turbulence of a manufacturing district, peculiarly unhappy from a very great deficiency of a middle rank, as there the population almost wholly consists of rich manufacturers and poor workmen; with whose minds no pains are taken by anybody; with whose afflictions there is no virtuous family of the middle rank to sympathize; whose children have no good example of such a family to see and to admire; and who are placed in the highly unfavourable situation of fluctuating between very high wages in one year, and very low wages in another? It is altogether futile with regard to the foundation of good government to say that this or the other portion of the people, may at this, or the other time, depart from the wisdom of the middle rank. (1820a, 32)

While these observations should not be taken to indicate that Mill wished to deny democratic citizenship to the working class, they are indicative of the character of Mill's argument *for* enfranchising them. For example, consider the remarks about needing to educate men and women to correct opinions and about having them emulate those "virtuous families of the middle rank" that Mill advocated. For Mill, it was essential to show that most of the time, most of the people "continue to be guided by that rank." It is also worth noticing that Mill claimed that these instances of popular unrest should, in fact, be regarded as an "exception" to the "rule" that the people were capable of judging. Indeed, Mill argued that these exceptions actually *proved* the rule: since there were so few of these protesters (measured against the whole population), "therefore" most must have been guided by those middling ranks of people of good sense. What is of importance here is that the practical locus of reform was tied to the people while the locus of the theoretical argument remained where it always had been, with appropriate knowledge, the interests of the community, and the standard-bearers of those interests.

KNOWLEDGE AND PROPERTY: THE POLITICAL COMMUNITY

It is evident, then, that whether in the context of his philosophical or practical discourse, James Mill uniformly advocated a government representative of a political community whose boundaries were to be drawn up according to one and only one criterion: knowledge. In this way, of course, Mill's argument shifted the focus of deciding the question of who should vote from its traditional eighteenth-century domain of inherent personal character with its origins in birth, rank, or breeding.[19] Instead, Mill located it squarely in the domain of the human mind—"the eye of plain reason" (1814, 2). The capacity to judge correctly one's own interest and to act upon it was all that was required. Rationality replaced reputation as the single most important qualification for citizenship.

In introducing this idea, of course, Mill had brought the doctrine of the equality of man into the science of politics in a way not open to the old virtualists among Mill's Whig contemporaries and predecessors. Unlike the virtues of the "great orders," in which eighteenth-century thinkers had placed so much faith, rational thought and action was potentially within the grasp of all. Even so, Mill's argument retained some vestiges of that earlier discourse. There still remained "the community" of shared and common interest, of which government alone was to take heed. However, since Mill's conception of rational action informed his understanding of the nature of such a community, much of what has previously been regarded as either confused or contradictory in Mill's politics falls readily into place. Given Mill's particular characterization of rational action, equality and political liberty came to fit easily with middle-class commitments to capitalist accumulation and their associated elevation of the values of frugality, prudence, and financial foresight (see Ryan 1972, 93). In this way, property should be understood as a "signifier" of appropriate knowledge, not a substitute for it. It was, so to speak, the enabling clause of Mill's science of politics, not its primary principle.

Among the most conspicuous instances of the association Mill drew between appropriate knowledge and middle-class understandings are those one encounters in his contributions to economic debate; and within that particular subset of his work, there are few sources more revealing of the general trend in Mill's argument than his *Elements of Political Economy* (1966, 203–366). His description of accumulation in the *Elements*, for example, bears all the hallmarks of his method of drawing out this association.

Proceeding along lines made familiar by Adam Smith, Mill asserted that the "augmentation of capital" was "every where exactly in proportion to the degree of saving" and that "the amount of that augmentation, annually, is the same thing with the amount of the savings, which are made annually" (1826a, 218). With savings behavior "therefore" holding the key to economic growth and prosperity,[20] which, in turn, required individual choices as to the allocation of current income between present and future consumption, Mill saw the whole matter as resolving itself into a simple problem of the felicific calculus—of balancing a present sacrifice (disutility) against a future benefit (utility). But this was a matter, of course, which had always hinged (for the utilitarians) upon individual knowledge. The more thorough the knowledge, so the argument ran, the more likely a time preference favoring the long view over the short. In his economic writings, Mill was fond of calling this the "disposition to accumulate" (see Mill, 1817a, 92–93; 1826d, 262),[21] and its utility to society was paramount. As he put it in his essay "Banks for Savings" for the Supplement to the *Encyclopedia Britannica*, "Human happiness is prodigiously improved by reserving for future use a proportion of the command which . . . a man may possess over the means of enjoyment" (2:92). The idea that appropriate knowledge was requisite to inculcating this propensity to save was, in fact, among the oldest of all of Mill's political maxims.[22]

It was when Mill turned his attention to discovering the existing seat of this knowledge that he was led to the middling classes, or (as he called them) the capitalists (1826d, 263). It was at this point that property entered his discussion of democratic reform. Viewed from this perspective, Mill's frequent references to the value to society of "many people of small to middling fortunes," in whom the "disposition to accumulate" was strong, were derivative of his idea that utility and knowledge were central to democratic political participation and not contradictory to it.

While it is easy enough to understand why his working-class Radical detractors should have found in that clause grounds sufficient for their determined opposition, it is imperative that Mill's position not be confounded with that of other middle-class reformers of the day. For Mill, unlike his philosophic–Whig contemporaries (and the authors of the Reform Bill of 1832), the ownership of property itself had nothing to do with establishing what James Mackintosh, in his well-known speech in the debate over the Reform Bill in 1831 called "moral trust" (1853, 2:385). Indeed, how much closer Mill's politics stood to that of his working-class radical detractors (and how distant it was from that of even the most advanced Whigs) may be measured by the fact that many of those theorists articulating the political aspirations of the working class actually took up Mill's principle (appropriate knowledge) and simply deployed it more aggressively to make their case for the inclusion of all adults into the political nation, without the need for a property qualification.

At the theoretical level, then, Mill had begun with a model of man unremittingly utilitarian in constitution. Starting with individual agents, he turned to his account of the science of human nature in his search for a science of politics suitable for the modern world. To make the case for the inclusion of larger numbers into the political nation, on this line of reasoning, all that needed to be established was that individuals had the capacity properly to judge their own interests and that they were unencumbered in their ability to act upon them. But if James Mill was an individualist in this philosophical arena, he was neither automatically nor necessarily a democrat in the political one. On that subject, everything turned on the question of judging when (or whether) individual capacity had reached an acceptable standard. This criterion proved to be sufficiently malleable to allow him to appear either expansive and democratic or narrow and elitist, as the case required. A recognition of this very basic element in the theoretical structure of the utilitarian theory of choice and action, (on which Mill's entire science of politics was grounded) is not without significance if for no other reason than that it appears to render the great debate over whether Mill was or was not a democrat "in principle" largely irrelevant to a clearer assessment of his political thought.

Notes

1. For perhaps the most recent and lively debate over the character of Mill's democratic commitments, see the exchange conducted between William Thomas (1969, 1971) and Robert Wendell Carr (1971a, 1971b).

2. For example, Asa Briggs remarked that the "paeans of praise" for the middle ranks of society that arose during that time "were echoed even more eloquently and certainly more permanently by James Mill in his famous *Essay on Government*" (1956, 68–69). Donald Winch would seem to concur, inasmuch as he claimed in his editorial annotations to Mill's *Selected Economic Writings* that "for all his talk of 'the people', [Mill's] view of the working-class movement was basically paternalistic; their best fate was to be guided, and perhaps ultimately assimilated, by their immediate superiors" (1966, 202; see also pp. 13, 196; but cf. Collini, Winch, and Burrow 1983, 101). See also, Grampp; 1948, 728; Gay, 1984, 40. This opinion, of course, did not have to await the advent of James Mill scholarship to gain expression. The charge that Mill was the champion of the interests of the middle classes against those of the working class was regularly brought against him by contemporary working-class Radicals (see N. Thompson 1984, esp. 31–33, 56). For Mill's opinion of one of their number (Thomas Hodgskin), see Mill to Brougham, 3 September 1832, Brougham Papers, Mss. 10765, University College, London (quoted in Robbins 1961, 135).

3. See, e.g., Burrow 1988, 40; Hamburger 1962, 188–190 Alan Ryan argues: [Those] identifying James Mill as the uninhibited apologist for capitalism that he has retrospectively been claimed to be ought to reflect on the fact that Mill's own view of what had to be avoided at all costs was what he called the unhealthy condition of the large manufacturing districts, where an impoverished work force confronted a harsh, selfish and money-grabbing group of employers. If democracy and property were to be compatible, the distribution of property had to be such that it diffused rather than provoked the class war" (1984, 113).

4. Consider Elie Halévy's *Growth of Philosophic Radicalism*, in which it is claimed that in the essay "Government," James Mill "was obliged to be careful concerning both the publisher who printed it, and the public to whom it was addressed" and "explicitly adheres in it neither to the principle of utility nor to the democratic idea" (1928, 420). See also Thomas 1969, 1979; he remarks that "a document which affords quotations to support all these interpretations must either be very rich in matter of very confused in argument" and claims that the essay on "Government" does, indeed, contain "two mutually contradictory positions" (1969, 250–51).

5. The editor of the recently published edition of Mill's political writings, Terence Ball, dismisses the charge of inconsistency by claiming that it stems from an "anachronistic" reading of Mill (Mill 1992, xxi). Mill, he claims, never meant "middle class' when he referred to the 'middling rank"; he had in mind, so it is claimed, the older eighteenth-century Scottish notion of "rank." We shall have cause to return to this point later. For the present it is enough to observe that such a rendering of Mill falls squarely into a new tradition of scholarship (see, e.g., De Marchi 1983; Haakonssen 1985) that seeks to anchor early nineteenth-century political discourse in the framework of concepts deployed by its eighteenth-century counterpart—a revisionist enterprise that is, itself, not immune to the charge of being anachronistic.

6. The conventional wisdom holds that James Mill began as a devotee of the Scottish moral-sense view (he had, after all, been at Dugald Stewart's moral philosophy lectures in Edinburgh in the 1790s) and converted to utilitarianism only after his meeting with Bentham in 1808. As Elie Halévy put it, "Bentham gave Mill a doctrine, and Mill gave Bentham a school" (1928, 251; see also pp. 154, 249–50 and Mill 1966, 7). The evidence mustered in support of this story involves comparing three of Mill's articles for the *Literary Journal* (1806b, d–e) said to be fully conformable to the Scottish tradition of moral sense with his article on Spanish America

for the *Edinburgh Review* and his review of Dugald Stewart's *Elements of the Philosophy of the Human Mind* for the *British Review* (Mill 1809, 1815), both of which are said to embrace the utilitarian position (see Halévy 1928, 257; Mill 1966, 7 and note). It is, however, worth remembering that Filangieri was himself heavily indebted for his argument to the Italian utilitarian Cesare Beccaria and that it is quite possible that the seeds of Mill's later single-minded devotion to utilitarian philosophy might well have been planted thanks to his encounter with Filangieri in 1806.

7. This, too, it should be said, was an old opinion of Mill's; the review of Filangieri in 1806 well illustrates his attachment to it. For example, in one place Mill writes (approvingly) as follows: "Montesquieu said that fear was the principle in a despotic government, honour in a monarchy, and virtue in a republic. Our author shows that these are only modifications of the same principle, the love of power" (1806c, 231). The whole idea is reminiscent of Hume's well-known remark that "political writers have established it as a maxim, that, in contriving any system of government, and fixing the several checks and controuls of the constitution, every man ought to be supposed a *knave*, and to have no other end, in all his actions, than private interest" (1752, 42)—which Mill brought to his own defence in the *Fragment on Mackintosh* (1835b, 279–81).

8. For Mill's expanded discussion of the morally corruptive effects of the open vote system, see 1830, 8–10.

9. Mill 1835a, 203–4; see also 1830, 1. Indeed, some have gone so far as to claim that the utilitarians "put all their eggs in one basket"—and perhaps the wrong basket at that—namely, the secret ballot. See Thomas 1969, 284; see also Carr 1971a.

10. Of course, not all democratic reformers had such sanguine views of the middling rank. Shelley for one, thought that they "poisoned the literature of the age in which they lived by requiring either the antitype of their own mediocrity in books, or such stupid and distorted and inharmonious idealisms as alone have the power to stir their torpid imaginations. Their domestic affections are feeble, and they have no others. They think of any commerce with their species but as a means, never as an end, and as a means to the basest forms of personal advantage" (1965, 7:29).

11. Mill (1826b) took the opportunity of his review of Samuel Bailey's *Publication of Opinions* to develop this argument (see also 1806a; 1812, 51; 1813; 1826c, 263; 1835d, 23).

12. Indeed, as William Thomas has noted, "At a time when moderate radicals were becoming more hesitant and popular radicals more extreme about the traditional radical demands for universal suffrage, shorter parliaments and the ballot, the *Essay* managed to evade all three" (1969, 257).

13. Whether Bentham's assessment of Mill's attitude was accurate or not, an important point of comparison emerges. It should not be too readily assumed that the two thinkers subscribed to the same analysis of either the theoretical or the practical limits of the expanded franchise. While Bentham's commentators, most notably Halévy (1928), have argued that Bentham's practical political reform positions were significantly less a product of any intrinsically democratic political thought than the necessary consequences of a utilitarian theory of action, few have questioned his preference for a virtually universal suffrage (1817 3:533–37). The same may not be said of Mill's argument in "Government." Despite opening the theoretical door to a "majority of the community," Mill never walks through it to universality. Neither an explicit discussion nor even the term *universal suffrage* appears in the essay. Indeed, even Mill's greatest critic, Macaulay, concludes that he would not have been completely satisfied with it as unambiguously serving the purposes of good government (1829, 409).

14. It is worth noting that while Mill professed a desire to see the interest of the community expressed in politics, for him, the community was one marked by shared, even homogeneous interests, and it existed only as an abstract ideal to be realized (constructed) in the future. This may help to explain the occasional expressions of frustration by John Stuart Mill—who shared his father's hope for the construction of this ideal community of interest—at the slow progress being made toward it during his own lifetime: "In England, it would hardly be believed to what degree all that is morally objectionable in the lowest class of the working people is nourished, if not engendered, by the low state of their understanding. . . . Few have considered how anyone who could instil into these people the commonest worldly wisdom—who could render them capable of even selfish prudential calculations—would improve their conduct in every regulation of life, and clear the soil for the growth of right feelings and worthy propensities" (1845, 511; on this point, see also Coats, 1971, 152–3).

15. We borrow this term from Bentham's *Plan of Parliamentary Reform* (1962, 3:522)—as did Mill (1829, 2:181–3) somewhat later.

16. This position should be contrasted with that of the Whig reformers of the day; see, e.g., Jeffrey, 1809, 1819; Mackintosh 1818, 1820.

17. William Thomas discussed Mill's argument for the disenfranchisement of women and children on these grounds, but does not discuss the logical difficulty (1979, 128). The same is true of Gregory Claeys (1989, 173). We are, of course, confining ourselves here to the logic of the case. Historically speaking, the utilitarians took up a variety of positions on exclusion (see, e.g., Ball 1980).

18. Having said this, it should be noted that Ricardo seems to have seen this particular matter (which concerns the logic of Mill's argument) rather differently. On reading Mill's essay in the summer of 1820, Ricardo wrote to Mill, saying that he found it "a consistent and clear development of your own views" (*Ricardo to Mill*, 27 July 1820, in Ricardo 1951–73, 8:211). Two years later, he sent a copy of the essay to Hutches Trower, recommending it as "an excellent article and well reasoned throughout" (*Ricardo to Trower*, 25 January 1822, in Ricardo 1951–73, 9:154). For a more detailed discussion of Ricardo's politics and its relation to James Mill, see Milgate and Stimson 1991, esp. 3–6, 21–23, 63–65, 69–70, 92–93, 96–99, 121–22.

19. Compare to this the views of Burke on the subject: "Men are qualified for civil liberty, in exact proportion to their disposition to put moral chains upon their own appetites; in proportion as their love of justice is above their own rapacity; in proportion as their own soundness and sobriety of understanding is above vanity and presumption; in proportion as they are more disposed to listen to the counsels of the wise and good, in preference to the flattery of knaves" (*Letter to a Member of the National Assembly*, in Burke 1839, 3:326). For Mill, there was just no need to put "moral chains" upon one's own "appetites" wherever intellectual aptitude was appropriate to informed choice.

20. This assertion, of course, also involves Say's Law: "Whatever is saved from annual produce, in order to be converted into capital, is necessarily consumed" (Mill 1826a, 326; see also p. 325). The point, then, is that capital accumulation actually requires not only that individuals save some portion of their current income but that they also invest those savings. Say's Law, in this domain of its application, conveniently allowed Mill to sidestep the relation between saving and investment.

21. Incidentally, this was a phrase John Stuart Mill would deploy years later in his own *Principles of Political Economy* in order to make exactly the same argument (see, e.g. 1871, 1.11.1–2).

22. See, e.g., the discussion of "frugality and industry" in Mill's thoughts on educating the laboring poor (1806a, 528–9, 533). See also his discussions of the relation between "prudent" action (which, for him, involved a present sacrifice for a future gain) and knowledge (1829, 2:282–3), the importance of education in fostering "foresight" and "self-command" in "Benefit Societies" (1817b, 263), and the need for designing a system of education to inculcate "temperance," (that is, "the steady habit of resisting a present desire, for the sake of a greater good" (1818b, 104).

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ANTONIN SCALIA, WILLIAM BRENNAN, AND THE POLITICS OF EXPRESSION: A STUDY OF LEGAL VIOLENCE AND REPRESSION

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The study of legal politics often attends to the description and explanation of the instrumental politics of legal change or to the mobilization of rights by individual citizens as an act of empowerment beneficial to their individual liberty. Drawing far less attention from the discipline is the privileged construction of a discourse or set of attitudes about rights by the judiciary. I present a case study of the First Amendment opinions of two members of the Supreme Court of the United States to criticize the range of their attitudes about rights and to illustrate how their opinions help construct and legitimate the disciplinary actions that provide order in the modern liberal regime. To preserve order, the justices are shown to use the language of rights as an instrument for the facilitation of violence, repression, and subjection against some litigants, rather than as an instrument for the enhancement of expressive liberties.

Often, the description and explanation of the instrumental politics of rights in relation to change in the politics of a regime preoccupies the study of rights by political scientists (see Epstein and Kobylka 1992). Also, the value of rights as a progressive instrument toward the attainment of human freedom and the mobilization of rights by individual citizens as acts of empowerment absorb other political scientists (see Zemans 1983). A darker vision of rights as an ambiguous practice that facilitates, through violence, repression, and subjection, injustice against litigants draws far less attention from the discipline. Political scientists have seldom attended to how the institutional arrangements of the polity allow the judiciary to create a practice of rights that might harm individuals. When they have addressed injustice and the repressive and violent aspects of rights, their commentary has usually maligned the lack of sensitivity to the interests of minorities in the application of legal rights (see Irons 1988). Only recently have political scientists and legal scholars taken a closer look at injustice and the repression and violence embedded *within* the doctrines and opinions that comprise the corpus of rights in a way that restricts the expression of other values or attitudes about rights (Cover 1986; Sarat and Kearns 1991). I aim to shed further light on injustice and the implications of violence and repression against individuals in the practice of American rights. After an introduction to the postulate that injustice is embedded within American rights, I offer an illustration of this postulate through the analysis of the opinions on First Amendment rights written by justices William J. Brennan, Jr. and Antonin Scalia of the Supreme Court of the United States during the Rehnquist Court.

DISCIPLINE, LEGAL ORDER, & RIGHTS

Nearly two decades ago, Stuart Scheingold (1974) illustrated how the "myth" of American constitutional rights served as a political resource for group efforts challenging the political status quo. He convincingly summarized how Americans viewed rights as a limit on legislative power and as a resource in litigation that could expand individual freedoms. Although interest group leaders have accepted the use of rights as a political resource, Scheingold indicated that interest group uses of legal rights did not always end with humane political change. Often, the judiciary was resistant to group strategies. Consequently, he concluded, "Law . . . serves the status quo in a kind of dual capacity. Legal processes are closely linked to the dominant configurations of power. At the same time, in its ideological incarnation, the law induces acquiescence in the established order by suggesting that the political system is beneficent and adaptable" (p. 204). John Brigham (1987), elaborating on Scheingold's ideas about law and the politics of rights, found that the language of rights defined the range of political actions and the resources available for political groups seeking greater freedom. Timothy O'Neill (1981) concluded an analysis of metaphors in constitutional law by noting that the legal language employed by the judiciary in equal protection cases had narrowed the issues considered in political equality debates, simplified complex political relationships, and impoverished public understanding of political controversies. Thus the constitutional law of rights prevented serious challenges to the inequality existing in the American polity. Scheingold, Brigham, and O'Neill sensed that judicial decisions about legal rights were often about the creation of stability and discipline in the polity, rather the expansion or protection of political liberty. They asserted that a constitutional law of rights for the protection of individuals against governmental authority can be transformed to support a few dominant attitudes about political power and to limit the agenda of claims about political liberties.

It is not surprising that the practice of American constitutional law could be construed as limiting claims to liberty. "Ordered liberty," a symbolic construction of the political universe, was a central aim of the authors of the Constitution (see Arendt 1965; Corwin 1948, 1–9; Hirsch 1992, 29–71). They desired liberty, or free political space for social interaction and the fulfillment of individual ends, but they also thought that liberty could be dangerous. Their abstract idea of liberty protected by a written constitution and judicial interpretations of the constitution presupposed the essential liberal position that the formation and self-direction of the individuality and sociability of persons—their emotions, intellect, property, and culture—occurs only within an orderly and secure political environment. Disorderly, pathological human behavior can abuse political liberty and pose a danger to the fulfillment of personal aspirations (Flathman 1989; Gaus 1983, 139–51; Pennock 1979, 17–35). Although liberty had various meanings for American founders, the most influential of them regarded arbitrary political decisions and the display of passion and self-interest as destructive of liberty (Hamilton, Madison, and Jay 1961, 56–65; Hirsch 1992, 41–51). Liberty—the liberty to establish normative diversity—was at the core of their politics.

Although the outcome of politics in liberal regimes is to be liberty, these regimes can generate a pathological political outcome that will be identified as injustice. Injustice is individual suffering resulting from institutional procedures for allocating goods and services or from the unethical or inequitable distribution of valued rights, entitlements, or possessions by others. Injustice is a consequence of the choice of political passivity when confronted by the plight of others and by actions that foreclose the essential individual and social fulfillments and attainments (Shklar 1990, 16–50). Liberals assume that injustice is suffering that citizens, public officials, and judges can reduce through political remedies. To control injustice, liberal regimes have sought to control or impose discipline on the political spaces where the tensional forces in society meet.[1] As a primary control on political space, liberal regimes adopt rules. These rules are based upon the notions of human reasonableness and the view that general principles must control arbitrariness. Because liberal regimes reject religion and tradition as methods for control of political space, the rules are constructed through representative political processes.

In the United States, liberal political conceptions mean that liberty can only flourish in political space if the corrupting pathologies are disciplined by the rules of a written constitution. Acceded to by popular conventions, the Constitution employs rules about fractional political conflicts and legalized political institutions like separated powers and federalism to discipline threats to liberty (Arendt 1965, 141–281; Hamilton, Madison, and Jay 1961, 56–65, 347–53; Wolin 1960, 388–93). Defenders of the American regime privilege these disciplinary rules; they revere their constitutions. Constitutional discourse has become the critical methodology for framing the appropriate rules to protect liberty. Carried into the American constitutional tradition by Alexander Hamilton and John Marshall, the rule-oriented style of discourse regarded the Constitution as a fundamental law containing, by consent, the intentions of the people about the control of political space. These proponents of legal discourse believed that law defended liberty by providing an institutionalized discipline on "the arts of designing men" and "serious oppressions of the minor party in the community" (Hamilton, Madison, and Jay 1961, 527).

Consequently, in a modern liberal regime like the United States, the law is a disciplinary institution. The law is designed as a generally applicable body of rules to discipline the chaos or uncertainty conducive to the pathology of injustice. Law aims at the discipline of a person's morality, character, social status, or psyche, and it is regarded as an absolute good because it is a rational instrument protecting the essential political requirement of personal liberty. For example, a recent defender of legality, Stephen Macedo, notes, "Law helps impart order to liberty by imposing on all free actions certain general conditions . . . and limits . . . that together create a system of mutual self-restraint harmonizing the freedom of each with the freedom of all. . . . Law stands for a certain kind of order: the mutual observance of general, public rules and procedures. Liberal law not only establishes the limits of power, it also provides a setting and structure for public justification. The impartial, politically independent institution of the court provides a most promising setting in which to approach the reasoned reflection required by justification (1990, 80–81)." As Macedo illustrates, the modern liberal regime assumes that the law employed by independent courts provides an impartial, reasoned, and justifiable solution to conflicts about the nature of personal liberty.

As with Macedo, most liberals assume that law breaking is an illegitimate effort to dominate political space and harm the essential liberties of individuals. A disciplinary law creates punishment for forms of disorderly expression. Deviation from the law means that an individual might have to submit to the authority of the state to penalize wrongdoing as defined in the body of rules. The disciplinary character of the law means violence or repression of some attitudinal expressions, but the defenders of liberalism assume that the violence of the law or related repression of behavior is legitimate when it is a judicially sanctioned penalty against abusers of individual liberty or perpetrators of private violence. However, despite its alleged legitimacy and rationality, there is a danger with a disciplinary law. The danger is that discipline might not be directed only at judicially certified wrongdoers. Thus liberal regimes constructed a law of rights to protect people against unjust discipline by the regime.

If we conceive of rights in liberal regimes as a practice—a set of actions grounded in rules (Flathman 1976, 11–32)—they serve as a mode of action governing political space over time.[2] As an activity, rights are an open-ended and politically constructed practice (Olivecrona 1971, 135–216; see also Cover 1986, 1610–13, 1617–18). In the United States, where the practice of rights is primarily a judicial duty,[3] judges interpret the legalized text of constitutional rights at the behest of claimants and apply the interpretation to disputes about liberty. Rights are a special practice designed to ameliorate the danger of an unjust discipline in other laws or governmental actions or private violence damaging the personal liberty of a claimant. As a practice, rights are constructed limits, or "boundaries," between liberty and order that define political space (Nedelsky 1991; see also Flathman 1976, 144–60). Rights are assumed to be a way of defending the political space necessary for individual expression and individual liberty.

VIOLENCE, REPRESSION, AND SUBJECTION IN THE PRACTICE OF RIGHTS

The practice of rights permits the use of judicial power to prevent the unjust discipline of persons and to foster participation in politics. However, despite the assumption that rights protect liberty, officials of a liberal regime can implement interpretations of rights that cause violence, repression, and disciplinary subjection against individuals who have not acted to harm or to offend profoundly other persons. Passivity to the sensibilities of claimants of rights or inattentiveness to the consequences of rights can result in injustices by officials, especially by the judiciary, which is primarily charged with the practice of rights. Also, "rights devised to give protection against the state may . . . be used by individuals against less powerful individuals" (C. Smart 1989, 145). A practice of rights by the judiciary that responds to powerful or legally adroit claimants can foreclose consideration of injustices through a policy of neglect or through choices harming the normative aspirations and interests of people. Therefore, to fully appreciate the practice of rights, we must examine how the practice of rights might allow the judiciary to lodge disciplinary decisions on people whose moral or social claims threaten the existing political order.

How do unjust disciplinary practices of rights by judges occur in liberal regimes? The answer lies, ironically, in the structure of the liberal regime. Since the regime is devoted to ordered liberty, its officials must practice rights in a way that accommodates liberty and order. They must use the power assigned to their office, through popular consent to a constitution or statutes, in a way that "cuts" (Derrida 1992, 24). In the practice of rights, judges' decisions cut because they divide, by calculation, attitudes about the proper scope of liberty into the acceptable and the unacceptable. The disposition of claims is their assigned duty and a practice that they are empowered to perform by the foundational political rules of the Constitution. It is an attribute of their official power. If the practice of rights in a liberal regime features the use of judicial power to cut or define legitimate and illegitimate attitudes about the practice of rights, judges might use their power in several ways. Besides using their power to write attitudes supportive of individual liberty into law, they might practice rights to advance order against liberty in four different ways—(1) to order a party to do something who would not otherwise set it as an objective (Dahl 1957); (2) to prevent a person from achieving an objective (Bachrach and Baratz 1962); (3) to manipulate a person or to shape preferences so that the person does something contrary to his or her objective, real interests (Lukes 1974, 21–25); and (4) to affect, through contributions to social practice and social knowledge, the definition of the identity, but not the objectives, of a person (Digeser 1992; Foucault 1980a, 81–98). The use of judicial power to discipline discloses a practice of rights that not only induces suffering or injustice but that employs power to ends other than liberty. For heuristic purposes, judicial use of the forms of power to unjust ends can be spaced along a continuum. Near one end are the practices of the use of power identified with command and suppression and identified as *violence*. Nearer the other extreme are the practices identified with manipulation and labeled *repression* and *subjection*.

To illustrate the interaction of power and injustice embedded within the practice of rights, I shall consider U. S. Supreme Court opinions on a question of rights, the space for expression provided by the First Amendment.[4] Opinions, an aspect of the practice of rights, signify the scale and importance of a decision about rights, and they give the practice of rights a historical importance and invest it with meaning and with the special power and permanence of the written word. The subject of analysis is all opinions about First Amendment rights by Antonin Scalia and William J. Brennan, Jr. from the start of the 1986 term to January 1993—the early Rehnquist Court. Among recent justices, the constitutional jurisprudence of Brennan and Scalia seems to offer the greatest saliency for the study of the practice of rights about expression, the most basic of requirements for the achievement of the essential position of liberalism. These justices represent the two predominant normative or "attitudinal" dimensions of the contemporary struggle to determine the scope of rights (Brisbin 1990; Segal and Spaeth 1993). Using the Court as "the site of competition for monopoly of the right to determine the meaning of the law" (Bourdieu 1987, 817), attitudes like those of Scalia and Brennan so dominate the debate about the practice of rights that they cut out other normative positions and often do violence or repress those persons who express alternatives.[5]

Antonin Scalia is an example of a judge attempting to isolate the judiciary as an alleged realm of reason free of policy bias (Brisbin 1990, 1991; Kannar 1990, 1991; Scalia 1987b, 1989a, 1989b). He is reluctant to let the practice of rights interfere with majoritarian choices of the personal freedoms contained in the Bill of Rights (Scalia 1987a; Strauss 1991). Thus he introduces a formal, categorical, and disciplinary law to reduce fears of political chaos, and he wants to tailor readings of constitutional rights so that they do not stimulate public opinion hostile to the interests of the judiciary (Burt 1991). In his many years on the Court, William Brennan's constitutional discourse disclosed a different practice of rights. Brennan believed that the practice of rights was an instrument for human use, not a set of abstract rules governing people. As an instrumentalist, he refused to be locked into absolutes or fixed interpretations of legal language. His development of the tiers-of-scrutiny concept for the evaluation of claims of rights signified his instrumental approach to rights (Heck 1987, 1174–81). Comparison of the attitudes of Scalia and Brennan about the practice of rights and the revelation of the violence or repression in their free expression of opinions should provide support for the contention that the discipline of expression and subsequent injustice are part of the practice of rights in a liberal regime.

PHYSICAL VIOLENCE

Physical violence is, whether legitimated or not legitimated by law, a means toward the recreation of political order and the redefinition of political space (Arendt 1970, 52–56; Benjamin 1978, 277–300). As a recreative process, physical violence is a process marked by the imposition of power, power that is colored by the vice of cruelty or inhuman treatment. Violence is more than extralegal activity; it is disregard for the liberty of others. It is the desecration of their values and their bodies, and it is the destruction of their personal freedom and moral and physical being, and it can be an effect of the practice of rights as an expression of judicial authority. Such violence includes a variety of destructive and injurious abuses of the bodies of others by judges (Baynes 1991; Garver 1968; Ladd 1991; Williams 1976, 278–79). When the judiciary employs its power to assign pain and coerce people to give up their liberty, it supports the exercise of physical violence by the state. Robert Cover first described how judiciary sanctioned physical pain or death against slaves and in criminal cases in order, paradoxically, to communicate a message about the consequences of deliberate harms to others. He claimed that the law must justify the use of violence to overcome social inhibitions about harming others. American judges have done this, he argued, by rigidly separating the act of legal interpretation of the law of the state from the enforcement of their interpretation through violence (Cover 1986, 1627; Sarat and Kearns 1992). The act of violence thus becomes justifiable because the proper interpretation of the language of the law demands it. The ability to blame the regime for the violence and not personally to commit it distances the interpreter from responsibility. There is not even a judicial need to wash one's hands of the matter to protect one's virtue, as Pontius Pilate did.

Beginning with this consideration, Austin Sarat and Thomas Kearns (1991) explore reasons for the effort of judges to conceal the physical violence accomplished by some of their choices behind a cloak of legal interpretation. The reason they offer is that the dominant mode of legal interpretation adopts a policy of "forgetting" violence. They find that Anglo–American jurisprudence is so caught up with the identification and explication of rules creating an objective or neutral law that it ignores pain (Sarat and Kearns 1991, 221–46; Cover 1986, 1629). The concern with the assurance of a reasonable and consistent normative content to the law, necessary for the preservation of a body of standards for the conflicts assigned to the judiciary, obstructed most judges from looking outward at the pain they inflicted. In this external arena, they regarded pain as but misfortune caused by the rational solution of legal conflicts; pain was not a product of the force of law (Sarat and Kearns 1991, 264–71; Dumm 1990, 31).

Justice Scalia has sanctioned the application of the criminal law against litigants' claims through an effort to define the First Amendment with limited "generality" or the location of an action in a category beyond the reach of tests of the constitutionality of statutes (Tribe and Dorf 1991, 73–80). Consequently, the scope of the First Amendment is limited to certain actions, and the regime can freely use its coercive police powers to impose painful penalties against other individuals' expressive acts. Coercive policing can result in the physical pain of incarceration in the violent milieu of American prisons. Several opinions indicate the painful nature of his reading of the restricted generality of the First Amendment. All were cases in which Brennan did not offer an opinion in response to Scalia.

When he served on the District of Columbia Court of Appeals, Scalia initiated his effort to delimit the bounds of free expression with the criminal law. The standard he developed in a Court of Appeals dissenting opinion (*Community for Creative Nonviolence [CCNV]* v. *Watt* 1983, 622–27, diss.) was to simply deny that certain expressive action fell within the pale of constitutionally protected expression. From his viewpoint, the state had to permit only conduct with a communicative character. Otherwise, criminal laws proscribing an action "for reasons having nothing to do with its communicative character need only meet the minimal requirements of the equal protection clause" (p. 622). In *CCNV*, a group sought a permit to conduct a sleep-in protest in a national park to protest the problem of homelessness. The Park Service denied the permit, citing a threat of the natural environment of the park. Scalia read the permit denial as reasonably aimed at protecting the quality of a park, and he determined that the Park Service had applied its rules in an even-handed fashion. Despite the incidental control of a form of expression, his argument was that the rules at issue in this case were not directed against demonstrations or against communication.

In *Employment Division* v. *Smith* (1990) Scalia's opinion for the Court provided a further definition of his views on the generality of the First Amendment. At issue was whether use of peyote, a hallucinogen, used as part of a religious ceremony by a member of the Native American Church, was protected by the free exercise clause. Scalia opened his opinion with a discussion of the text of the free exercise clause, and he very quickly moved to read the text to protect conduct as well as belief, repudiating a distinction first established over a century ago. Then, with little more discussion of the history of the clause, he turned to consider the generality of precedent cases in which the clause was used to protect categories of religious belief, profession, and performance or abstinence from conduct. He distinguished the precedents from his position that drug use as part of a religious ceremony could not excuse violations of neutral, generally applicable criminal laws not specifically directed at a religious practice. He concluded that precedents held that physical acts performed for religious reasons, like peyote consumption, could be regulated by laws "not specifically" designed to prohibit a religious practice. According to Scalia, "We have never held that an individual's religious beliefs excuse him from compliance with an otherwise valid law prohibiting conduct that the State is free to regulate" (p. 1600). He continued by noting that the Court had made exceptions to this standard only when a neutral, generally applicable law involved free exercise in conjunction with freedom of speech or freedom of the press. Except for these "hybrid" situations, which he chose to distinguish from simpler claims of protection of religious conviction under free exercise "unconnected with any communicative activity or parental right" as in the unemployment cases, he made it clear that the scope of the free exercise guarantee was limited by laws established by the democratic majority to prohibit socially harmful conduct (pp. 1599–1602). Provided that the specific conduct of a specific religious association was not singled out for restriction or penalization, he left the definition of socially harmful conduct to the legislature.

Scalia supplied two reasons for his refusal to exempt Smith's religious exercise from the general criminal law. To exempt a religion from the criminal law, he said, "contradicts both constitutional tradition and common sense." Through his constitutional tradition argument, Scalia meant that Smith's claim was not comparable to those race and speech cases in which strict scrutiny and the compelling interest test had been applied in the past to exempt persons from governmental control: "What [the test] produces in those other fields—equality of treatment, and an unrestricted flow of contending speech—are constitutional norms; what it would produce here—a private right to ignore generally applicable laws—is a constitutional anomaly" (*Employment Division* v. *Smith* 1990, 1603–4). His commonsense argument was that allowing exemptions under a compelling interest test would afford "the prospect of constitutionally required religious exemptions from civic obligations of almost every conceivable kind." The result would be a threat to the rule of law that would be "courting anarchy." He also claimed it would force judges into the unsound practice of deciding whether a religious practice was so central to a faith that governments had no compelling interest to regulate it. Instead, he concluded, it was better to leave some religious conduct and belief relatively disadvantaged than to leave religions to become either privileged and a law unto themselves or subjected to judicial consideration of the merits of their beliefs (pp. 1601–3).

In *Barnes* v. *Glen Theatre* (1991), Scalia's concurring opinion extended the logic of his *Smith* opinion on the generality of the First Amendment to another form of expression. The *Barnes* plurality upheld an Indiana regulation banning nude dancing through the use of rational basis analysis derived from *United States* v. *O'Brien* (1968). However, Scalia wanted to ban the nude dancing because it violated a traditional public immorality statute, a law he deemed to be "a general law not specifically targeted at expressive conduct." Moreover, this statute was constitutional because it was "not specifically targeted at expressive conduct." Only when a statute prohibited" conduct *precisely because of it communicative attributes*," like flag burning or silent sit-ins, would it be unconstitutional. Referring back to *CCNV* and *Smith*, he again indicated that unless the government directly denied the ability to convey a message, restrictions of an "incidental" nature on expressive conduct framed as criminal laws of general applicability would be immune from constitutional challenge (*Barnes* v. *Glen Theatre* 1991, 2465–66, conc.).

By limiting the generality of the First Amendment to a narrowly defined range of expressions and by refusing to recognize the communicative nature of almost all human actions, Scalia tried to open a sphere for coercive state regulatory actions. When he conceded the regulatory power to legislatures in sleep-in, peyote, and nude dancing cases, it led to probable physical violence against the expressive parties. Persons became prisoners if they slept in, ate peyote, or danced without a G-string and pasties. If they acted out attitudes about politics, religion, and sex, the harm of incarceration in the physically violent world of an American prison faced them. Scalia would have placed their bodies in jeopardy, if not in pain.

SYMBOLIC VIOLENCE

Symbolic violence distinguishes a situation when people perceive that the authority of the law turns them into subjects of politically dominant groups or institutions (Bourdieu 1977, 83–197 and 237, n. 47; Bourdieu 1990, 122–34; Bourdieu and Passeron 1977, 3–11; Thompson 1984, 42–72). Consequently, it is a relationship in which the power of the judiciary is exercised to prevent people from exercising their liberty and achieving personal objectives. The effort at their subjugation originates within the arbitrary action of the judiciary. Despite efforts by the judiciary to conceal their control through symbolic devices, like the language of legal impartiality, members of the subjugated group consciously perceive the domination. They are conjoined in their own domination because they fail to recognize the origins of suffering inherent in their situation. They remain unwilling to challenge the myth of impartiality because of a fear of uncertainty, internalized dispositions about the nature of reality or legality, or fear of social conflict if the reality of their lack of power is verbalized. Thus the power of the judiciary to assign blame for unequal liberties empowers them to deflect the cause of suffering from elites. Through the production of a symbolic order of rights, judges can then assign blame for restrictions on liberty to misfortune or to the subjugated group's behavior. Thus a right to free speech is curtailed not because of a judicial decision but because a party threatened immediate civil violence contrary to a rule of precedent. The myth of the impartiality of the constitutional law of rights provides legitimacy for the hegemony of dominant groups and curtails political discourse about the suffering of the disadvantaged (see Young 1990, 96–121). Also, laws and judicial opinions consecrate and reproduce the relationship of domination in new contexts. Symbolic violence thus attempts to destroy the independent interpretation of the world by the subjugated group, its attitudinal independence. It censors any political discussion of their suffering.

Symbolic violence exists in several specific juridical acts, including opinions that encourage exploitation, marginalization, powerlessness, and cultural imperialism of groups of individuals. Exploitation exists when a group is deprived of material benefits or economic status and the deprivation is defended through legalisms employed by the judiciary. Marginalization is the relegation of a group to an inferior status. This marked group then faces denial of equal material or political rights. Powerlessness is the inability to participate in public policy choices and the selection of political leaders. Cultural imperialism is found in laws that perpetuate the culture of a group while restricting the expression of cultural values by other groups. The perception of symbolic violence causes individuals or groups to react by expressions of rage at government or subversion and insurgency of the politically dominant.

Although Brennan's doctrinal language attempted to defend the disadvantaged, Scalia has written several opinions that permit symbolic violence by government or private interests against the politically less advantaged. The symbolic violence includes opinions that prevent or harass opportunities for expressive freedom, including opinions that support exploitation of political communication by the wealthy, the disempowerment of selected political participants, cultural domination by a religious viewpoint, and marginalization of sexually oriented expressions. He justified the symbolic violence through a reconstruction of strict scrutiny doctrine. For Brennan, strict scrutiny was an interpretative instrument for the protection of the rights of selected disadvantaged groups and the defense of expressive freedoms of discrete and insular minorities. His reading of strict scrutiny doctrine required that the government must show how a challenged policy serves a crucial or "compelling governmental interest," is a substantially effective means of furthering the compelling government interest, and is a necessary and the least restrictive alternative for achieving the governmental interest (Galloway 1988, 453–55). Any limitations upon or disadvantages to the fundamental rights of a classifiable group of people must be closely examined by courts or given "strict scrutiny." Brennan originated the idea that a limitation on a First Amendment freedom or equal protection of the law often required the application of the compelling interest standard to permit the judiciary to protect a disadvantaged group from the effect of majoritarian policies (*Shapiro* v. *Thompson* 1969; *Frontiero* v. *Richardson* 1973). However, when internal divisions within the Court dictated it, Brennan practiced less intensified scrutiny and applied a less stringent test for the evaluation of evidence of governmental interference with rights and or the creation of an unequal classification. To meet contingent circumstances, he decided that important governmental objectives served by a classification and the substantial relationship of the classification to those important objectives ("intermediate scrutiny," *Craig* v. *Boren* 1976) or the reasonableness of governmental action ("nondeferential rationality review," *Plyler* v. *Doe* 1982) permitted using standards other than strict scrutiny and the compelling governmental interest test to protect the rights claim of a group.

In contrast, Scalia's reconstruction of scrutiny doctrine aimed far less at the defense of the interests of traditional discrete and insular minorities. In *Austin* v. *Michigan Chamber of Commerce* (1990) the Court upheld a Michigan law permitting the state government to control independent political campaign expenditures by corporations as serving a compelling state interest in preventing political corruption. In rejecting the evidence of a compelling interest necessary to permit regulation under the strict scrutiny standard, Scalia claimed that the Court made two bad arguments. First, he rejected the idea that the government had greater authority to regulate speech by a corporation because it had been given special advantages by the state. Speech, he contended, cannot be more readily restricted because the speaker has special advantages. Second, he argued at length that the massive wealth of corporations did not on its own make them likely to corrupt politics. By declining to view great wealth as a potential source of corruption and wrongdoing, he countered the compelling interest argument of the majority and stated that strict scrutiny demanded the protection of corporate expression. Further, he refused to accept the idea that any potential for harm by corporations justified a sweeping prevention of corporate political speech. In conclusion, he launched into dicta criticizing the majority for attempting to engineer equality in speech rather than letting the people, in the marketplace of ideas, "separate the wheat from the chaff" (pp. 1408–14, 1416–17, diss.). Although Scalia appeared to defend speech in *Austin*, his opinion ignored and concealed the violence of domination. The decision delimited an individual's ability to gain information about politics and make informed choices. Scalia stilled discussion, an essential feature of democracy, because he refused to recognize the effects of economic power on communication. He assumed that any virtuous citizen should be able to find out about politics; he neglected how the exploitation of communications by economic interests tended to skew information costs and prevented citizens from ready access to all political discussions. In effect, he took the strict scrutiny test and converted it from a protection of the disadvantaged into an instrument for the exploitation and expropriation of political discussion and the policy agenda by the powerful, and he sanctioned private efforts to destroy the ability of disadvantaged citizens to make an independent interpretation of the political world.

Brennan's concurring opinion in *Austin* was an extension and clarification of his earlier opinion in *Federal Election Commission* v. *Massachusetts Citizens for Life* (*MCFL*) (1986), which held that a limitation on corporate campaign spending could not be applied to a small antiabortion group. In *MCFL*, he determined that although government had a compelling interest in regulating campaign finance to prevent political corruption, the activity of MCFL did not offer a compelling interest for regulation. In contrast, the activity of the Chamber of Commerce did offer the potential for corruption, as identified in the opinion of the Court. Also, he specifically criticized Scalia's argument that political contributions by corporations offered only the potential for corruption and that they should not be regulated unless harm is clearly established. Brennan countered with the argument that regulation protected corporate investors from having their money used "to support candidates with whom they do not agree" and was similar to other aspects of the extensive state regulation of corporate for-profit and nonprofit activities. Consequently, the mere fact of Chamber of Commerce and corporate contributions "forced" the subsidization of speech by unwilling members or stockholders (*Austin* v. *Michigan Chambers of Commerce* 1990, 1402–7, conc.). Brennan thus recognized a form of the use of power—a symbolic violence—that Scalia bypassed thorough his attention only to the coercive power of the state.

Although Scalia converted strict scrutiny to foster private exploitative ends, he also has rejected its use when it promoted the political power of individuals and against established organizations. In *Tashjian* v. *Republican Party* (1986), Scalia dissented from an opinion of the Court that used the compelling interest test to strike down a law requiring that voters in a party primary election be registered members of the party. The Court determined that the law unconstitutionally burdened the right of the Republican party to admit nonmembers to its candidate selection process and that the law offered no compelling reason for the burden. Scalia thought that the Court had "exaggerated the importance of the associational interest at issue, if indeed it does not see one where none exists" (p. 559, diss.). Instead, he read the associational contact to be casual and the regulation to pose no burden on this brief encounter.

In *Rutan* v. *Republican Party* (1990), Scalia dissented from a Brennan opinion for the Court that held that political party membership was not a permissible factor in the dispensation of government jobs, except for a category of confidential policymaking jobs. Brennan determined that the constitutional right to freedom of association could not be set aside when the party provided no compelling interest for the maintenance of the preference on associational membership required by a patronage system of public employment. Scalia's dissent attacked the majority and the series of earlier Court decisions against the patronage system by arguing for the use of a deferential rational basis test. The reason he gave for diminished scrutiny was that governmental employment has long featured restrictions on employee speech and behavior subject to rationality test analysis. Further, he contended that by the use of a rationality test, patronage employment would survive because it is a traditional practice long deemed reasonable and because it plays a useful purpose in fostering parties, the two party system, and electoral participation (p. 2749–56, diss.). In a footnote, Brennan dismissed this argument, arguing that the interests that Scalia thought were promoted by patronage "are not interests that government has in its capacity as an employer. . . . Therefore, even were Justice Scalia correct that less-than-strict scrutiny is appropriate when the government takes measures to ensure the proper functioning of its internal operations, such a rule has no relevance to the restrictions on freedom of association and speech at issue in this case" (p. 2735, n. 4).

When the Court's majority refused to allow a selective, content-based taxation of magazines, Scalia dissented because he believed that the Court should not have applied strict scrutiny criteria to subsidies administered through exemptions from a tax. Such indirect subsidization of publication was not a restriction or prohibition on free press, he claimed, and it did not have a coercive effect on protect rights. Fearful that the use of heightened scrutiny would cause free expression challenges to direct or indirect governmental subsidization of bulk mail, the arts, and public broadcasting, he preferred the use of a rational basis test (*Arkansas Writers' Project* v. *Ragland* 1987, 1730–32, diss.).

In recent decades, the Court has applied "intensified" standards akin to strict scrutiny to some governmental efforts to curtail or control publications while using other tests for student and commercial publications (compare *Freeman* v. *Maryland* 1965 and *Lakewood* v. *Plain Dealer* 1988 with *Central Hudson Gas and Electric* v. *Public Service Commission* 1980 and *Hazelwood School District* v. *Kuhlmeier* 1988). Also, courts can set sexually oriented materials outside the boundary of constitutional protection by finding them obscene, but the test of obscenity demands intensified scrutiny of the publication (*Miller* v. *California* 1973). In a few opinions, Scalia addressed the difficult problem of whether obscenity rulings are a form of symbolic violence designed to marginalize by applying the label "obscene" to the ideas of selected individuals. The label of obscenity symbolizes the degradation of an idea, and courts have used obscenity to marginalize authors with a distinctive literary voice like Theodore Dreiser, D. H. Lawrence, and James Joyce. The problem is that some ideas degrade individuals. The Court, concentrating on such degradation, has devoted its opinions to the development of constitutional definitions of obscenity that marginalize the purveyors of pornography.

Scalia's opinions on pornography exhibit a willingness to regulate some of the offensive, as well as the harmful, varieties of pornography by the use of reasonableness, rather than intensified scrutiny. In the dial-a-porn case (*Sable Communications* v. *Federal Communications Commission* [FCC] 1989, conc.), he concluded that the FCC could not ban these services unless the message was proven to be obscene. However, he agreed that a more narrowly tailored and reasonable ban aimed at harms affecting minors (presently not technologically possible) might survive. Brennan's opinion in this case determined the ban to be unconstitutionally overbroad,[6] and he refused to accept any restriction on the distribution of obscene materials to be constitutionally tolerable (pp. 2840–41, conc. and diss.). However, in a Dallas case about the constitutionality of an ordinance that restricted the location and licensing of sexually oriented businesses, Scalia concurred in part and dissented in part from O'Connor's plurality opinion (*FW/PBS* v. *Dallas* 1990). O'Connor's opinion provided a complex answer to the validity of elements of the Dallas ordinance. Scalia opted for a simpler standard to control offensive, as well constitutionally unprotected, obscene materials by resurrecting the pandering standard of *Ginzburg* v. *United States* (1966). He would have construed the Dallas ordinance as a valid, narrowly drawn, and reasonable effort to prevent the unconstitutional distribution of sexual material. He argued that the state regulated the sexually provocative aspects of the distribution of the material. The law was not an overbroad regulation of a wide variety of expressions; it restricted only those enterprises that sold materials with a salacious appeal. He also failed to demand intensified scrutiny of the publication. Thus, he effectively marginalized the business practices of "adult" enterprises without having to employ an obscenity standard, rationality review, or a strict scrutiny standard. Besides punishing the sale of obscene pornography, Scalia's treatment of obscenity law marginalized and directed symbolic violence at the entrepreneurs vending offensive sexually provocative materials and displays. Additionally, Scalia apparently desires to marginalize offensive but not necessarily harmful acts. In *Pope* v. *Illinois* (1987), White's opinion for the Court held that the literary, artistic, political, or scientific value of allegedly obscene material was to be determined by reference to a reasonable-person standard, not the standards or ordinary members of a given community. Concurring, Scalia raised doubts about the worth of this literary-value prong of the Court's test of obscenity. He suggested that it relied too much on discretionary judgments of taste and lacked enough precision to forewarn publishers. An obscenity test without the literary-value standard, which demands an intensified scrutiny of a publication, might marginalize all sorts of offensive but not harmful publications and creative works in the category of constitutionally unprotected materials. But rather than propose an alternative, Scalia simply indicated a need to reexamine the test of obscenity (p. 1923, conc.).

The thrust of Scalia's use of strict scrutiny language was to advantage existing centers of political power, like the political action committees in *Austin*, party leaders in *Rutan*, and party activists in *Tashjian*, not the disadvantaged interests that Brennan attempted to defend through strict scrutiny. This tactic meant that Scalia varied his use of strict scrutiny to accommodate ends that meant the powerlessness of the political party outsider and the less wealthy or organized participant. Scalia used the strict scrutiny criteria only once—in an effort to preserve organized, corporate power. Contrary to the approach of Brennan, his opinions signaled symbolic violence through the reassertion of the politics of domination. They contain within them a symbolic message of violence directed at the expression of ideas by the politically disadvantaged or those persons who reject certain religious values privileged by legislative majorities. The practice of rights, in his hands, would become an instrument continuing the domination of persons by existing centers of economic wealth and institutional power within the regime. The limits of rights would thus restrict the ability to the poor, the political outsider, the secularists, and persons with a different sexual curiosity from pursuing their interests.

MANIPULATIVE REPRESSION

Repression is the process by which the holders of power exclude unacceptable desires or impulses from expression in political space. Repression exhibits disregard for the liberty claims of others, but, unlike violence, it disciplines the use of political space without coercion of the body or the psyche. Instead, in one form, identified here as *manipulative repression*, repression involves the use of power to manipulate persons so they do not make claims to liberty. Through law they are taught to "accept their role in the existing order of things, either because they can see or imagine no alternative to it, or because they see it as natural and unchangeable" (Lukes 1974, 24).

Alexis de Tocqueville first outlined the nature and sources of manipulative repression. It is the subjugation of people's attitudes to those of governmental leaders and arises when the people are equal and interested in the procurement of the "petty and paltry pleasures, with which they glut their lives." In this situation, government becomes an "immense and tutelary power, which takes it upon itself to watch after their fate." Through laws, the government proceeds to ensure public order and economic security and becomes a "shepherd" that regulates society through numerous rules. Tocqueville depicted a regime in which the public has let itself be subject to domination. The public accepted the hegemony of disciplinary law because of the sense of order and security it provided in an uncertain world (1945, vol. 2, pp. 334–39). The law inhibits action; it bends wills toward the goals of the political administration. For Tocqueville, the concept of equal rights might prevent rule by tyrants and despots, but it strengthens passivity in the face of a disciplinary law. Equal rights protect people and allow them to satisfy personal material needs, but equal rights also isolate people and make them think of their own interests. This allows governmental officials the space to initiate uniform legal controls in the name of equality without opposition from an isolated citizenry unconcerned with civic life. In this setting, passive injustices flower. People act with little attention to the interests of others, and they leave the amelioration of harms and suffering to the administration and the courts (see Boesche 1987, 229–59; Dumm 1987, 128–40).

When laws are legitimate, the government, especially the judiciary, can define the language of a right to exclude recovery for repressive acts. The response of affected individuals to their exclusion is resignation. They suffer, but they do not rebel. Rebellion would threaten the social peace assured by the shepherd state and the habit of dependency. The brief stories of females subjected to gender discrimination retold by Kristin Bumiller (1988) exhibit a related dimension of manipulative repression. For Bumiller, manipulative repression persists not just because of people's habit of deference to the law but because of fear of the law. They fear that if they seek a legal resolution, they will not improve their position but will lose control of a hostile situation (p. 109). For these women, the superintending power of the law left isolated individuals adrift and in fear of seeking any satisfactory relief for the repressive desecration of their lives by discrimination. Fear of the submission of their claims to the judiciary shepherded these women into suffering.

Often, the Court defends the "rationality" of governmental action to evaluate competing claims about rights and to encourage deference to centers of political authority. This form of the review of statutes assumes that "governmental action is constitutional if it has any conceivable valid purpose and if the means chosen are arguably rational" (Galloway 1988, 451). The rational basis test appeared in several cases in which Scalia wrote opinions; however, the Court's majority used the test in a manner not deferential to the government. In contrast, several of Scalia's First Amendment opinions privilege the tutelary power of the state through the use of a deferential rational basis test. His use of the test would readily permit governmental manipulation of the dissemination of information and confirm the quiet governmental direction of peoples' lives in ways often contrary to their interests.

Manipulative repression occurred in Scalia's use of the rational basis test in his opinion in *Rankin* v. *McPherson* (1987). The Court determined that a First Amendment right to make a statement on a matter of public concern outweighed any reasons for the dismissal of McPherson, a clerk in a Texas constable's office, because of a remark indicating that she hoped that someone might assassinate President Ronald Reagan. Scalia dissented. First, he considered that McPherson's statement failed to satisfy the threshold requirement to qualify as a speech on a matter of public concern. He argued that in its content the statement was dissimilar with other statements the Court had deemed to be on matters of public concern and that it was closer to statements the Court judged to be entitled to no First Amendment protection. In making this judgment, Scalia refused to inquire into the context in which McPherson made the statement, and he treated it like statements similar in content but made in an environment of imminent disorder, as in the "fighting words" cases. Second, he assessed the weight of the constable's reasons for dismissal differently from the Court. Scalia thought that his interest in "maintaining both an esprit de corps and a public image consistent with his office's law enforcement duties outweighs any interest his employees may have in expressing on the job a desire that the President be killed" (pp. 2902–5, diss.).

Scalia wrote for the Court in a case where a state university had enforced a regulation on the use of school property to exclude a person from holding a Tupperware party in a dormitory room. Scalia held that the regulation of the party as commercial speech did not have to satisfy a least-restrictive-means-available test to survive as a regulation of speech. Instead, the regulation was legitimate if the government's goal was "substantial, and the cost to be carefully calculated." Additionally, since the validity of the statute had not been decided, he refused to determine if the regulation was overbroad until the trial court ruled on the validity issue (*Board of Trustees* v. *Fox* 1989, 3035–38). Also, Scalia manipulated repression directed at discussion. He wrote a brief opinion concurring with the Court's opinion defining the scope of licensing and other time-place-manner restrictions on charitable solicitations. Scalia's only objection to the Court's opinion was with language in one of its footnotes. However, his objection signified his reluctance to allow states to impose even a narrowly tailored disclosure requirement on professional solicitors (*Riley* v. *National Federation of the Blind* 1988, conc.). Finally, concurring in a decision to curtail expressive activities on the streets near polling places, Scalia rejected the strict scrutiny approach of the majority. Instead, he simply found the curtailment to be a reasonable control, because the street on election day lost its character as a traditional public forum (*Burson* v. *Freeman* 1992, 1859–61, conc.).

Scalia's decisions about the reasonableness of restrictions on expression culminated in *R.A.V.* v. *St. Paul* (1992). In a conflict about the constitutionality of an ordinance punishing the expressive display of symbols known to arouse "anger, alarm or resentment in others on the basis of race, color, creed, religion, or gender," his opinion for the Court ignored the strict scrutiny approach. Instead, he attempted to see if the statute fit into any categories of speech that could be proscribed. After a recapitulation of the categories, he determined that the ordinance was not permissible as content regulation and that it was an action of unreasonable viewpoint discrimination. He rejected the argument that the statute was a narrowly tailored regulation of speech designed to further a compelling state interest on the grounds that the ordinance was not reasonably necessary to further any interest other than a city council's hostility to a particular idea. Although his opinion did not repress the vicious expression of a hateful viewpoint, it left the potential for the state reasonably to repress other categories of expression because of content.

Although the Scalia opinions reflect key political assumptions of the justice, including deference to legislative choices and the will of the majority, they all fail to consider the repression buried in deference to the majority's notion of civic order. In these cases, the innocent if offensive comments about a political figure, an enterprise to raise money for college expenses, and the use of professional fund solicitors by a disadvantaged group all become the subjects of state control.[7] Scalia's opinions manipulated individuals into suffering because they required deference to the interests and attitudes embedded in a tutelary law. His opinions made it difficult for parties to challenge in atypical ways the authority of the state. Embedded in the opinions of Scalia was repression of expressions that offended but not harmed others. Scalia wanted these litigants to resign themselves to guidance by the "reasonableness" of the decisions of the legislative majority and give up their liberties. They were to accept roles of subordination that, in his eyes, were natural and rational.

Brennan, expressing his ideas less frequently in opinions, sought to curtail repression by using the language of heightened scrutiny to protect the disadvantaged group. Yet he still implied that the scope of expressive liberty was subject to judicial power. Brennan's opinion for the Court in *Riley* employed strict scrutiny to determine that compelled disclosure and restrictions on fees of professional fund-raising firms were a much more serious threat to the interests of the National Federation of the Blind because they were "imprecise," "unduly burdensome," and not narrowly tailored enough to serve a compelling interest in the prevention of charitable fraud (*Riley* v. *National Federation of the Blind* 1988). When the Court restricted the right to free expression in high school newspapers, he criticized the reasonableness test and the evidence used to support it. He would have applied a stricter, but not the highest, form of scrutiny to ascertain the legality of official's regulation of the newspaper (*Hazelwood School District*, v. *Kuhlmeier* 1988, 573–80, diss.). His opinion in a commercial speech case curtailed state regulation of advertising by lawyers. He recognized the advertising as expression protected by the first amendment, but under a special body of doctrine requiring less strict scrutiny (*Shapero* v. *Kentucky Bar Association* 1988). Brennan denied claims of free expression only once. As noted, his *Austin* concurring opinion supported coercive control of corporate expression. However, this decision implies that he would continue some subjection of expression to the dictates of the Court.

SUBJECTION

Subjection, or accountability to the judgments of others, recognizes that power exists in interactions, and it is the interactive use of power that defines the normal.[8] As defined by Michel Foucault, the situation of subjection does not involve an authority, like the judiciary, seeking an objective interest. Instead, power exists as signs and symbols and beliefs about the political order. Persons are not subjected to power. Rather, power is the internalized discipline of a society, for "power comes from below" (1980a, 94). Foucault argues that this practice is less repression than the strategy of arranging political space through disciplinary norms in a work on conflicting power relations.[9] Regardless, the judge and others are involved in a strategy further to encourage the arrangement, standardization, and normalization of power relations. The judiciary is to "ensnare" popular justice, "to control it and to strangle it, by re-inscribing it within institutions typical of the state apparatus" (Foucault 1980b, 1; see also Foucault 1980a, 144). Judges and other participants in litigation thus reduce the danger of everyday political conflicts by interpreting rules in ways that people internalize and silently accommodate in their everyday lives (Foucault 1987, 33–40).

As a particular disciplinary use of power by the judiciary, the practice of rights becomes a way of marking or inscribing human behavior as normal and part of the political order. Therefore, judges attempt to establish a privileged discourse of imagery and rhetoric about rights. When formalized and institutionalized, the discourse can become a legitimated structure of illegality that forbids or isolates contrary discourses about power and liberty. Although the establishment of a legitimate, privileged discourse about rights reduces the frequency of making cutting choices about the discipline of people, judges still have one job.[10] Judges use their special knowledge of rights to rationalize and enforce, as normal, the power relationships that deploy people as subjects of the state or other powerful institutions and social interactions. Resistance by individuals to a practice of rights as an active discourse about power relations results in the judiciary's assigning the label of "different." The person who rejects the judicial practice of rights is irrational, delinquent, or an outlaw. Such behavior has transgressed the acceptable conditions of truth or reason embedded in social and political discourse, and the body must be disciplined. Judges then constrain such persons by prohibition, or a practice of rights that defines their behavior as beyond the boundary of liberty fixed by rights (Foucault 1977, 16–24; Foucault 1991, 75; see also Dumm 1990, 40–42). Also, they can use rights to extend their discipline to new claims about liberty (Simon 1992), and they support efforts of the state to induce order into political expression.

Although not always influenced by Foucault, critics have condemned the failure of the American practice of rights (especially readings of the equal protection clause) to recognize the importance of "difference" and to include the "different" within political space (Hirsch 1992, 115–240; Minow 1990; Young 1990). From their perspective, the practice of rights has used rules to exclude persons with different attitudes, including women, minorities, children, homosexuals, and the dying, rather than to devise practices of rights that permit the inclusion of their claims of liberty within the boundaries of legitimate politics. They perceive the definitions of rights to be a power strategy by the judiciary that furthers engendered or other language used by various power centers to discipline a social group and protect their own privileges. Thus these scholars condemn the Supreme Court's failure to enhance gender equality through expanded protection on abortion and strict scrutiny of discrimination against women, failure to end minority inequality through redistributive policies like affirmative action, and failure to attend to the needs of children and the concerns of the dying. They view these decisions as emblematic of the use of the practice of rights to construct a discourse of subjection.

Scalia and Brennan's freedom-of-expression decisions often cut in ways that left individuals in a situation of subjection to the power of the regime and to private power. When these justices' opinions identified individual expression as normal or different, they defined the contours of discipline in social relations. This discipline was not coercive or manipulative; instead, it was embedded in a practice of rights that legitimated dispersed locales of power and their domination of other persons. The Brennan and Scalia opinions reinforced two attitudes and related legal constructions about the legitimate use of disciplinary power to define the space for expressive liberty.

Subjection by the Regime

The practice of rights by Brennan and Scalia left intact various practices that privileged certain categories of expression, while the same opinions indicated how the concept of "illegality" could be deployed by the regime to denote the difference of another party. Therefore, the liberty of parties became subject to rules adopted by the Court, rules that increased accountability to court-imposed boundaries on behavior. As liberty became a judicial designation, courts came to shape the identity of persons and define their subjection. For example, Brennan's freedom-of-expression opinions and a few Scalia opinions used variations on the concept of heightened scrutiny to exclude certain forms of expression, like "fighting words" or exhibitions of child pornography, from constitutional protection. Also, the mere existence of the scale of scrutiny implied that some forms of expression were different and outside the space of liberty. In this unprotected space, the judicial practice of rights permitted the exercise of power in its violent or repressive forms and subjection by the political regime.

Brennan's view of free expression allowed for decisions that cut and permitted discipline by the regime in three ways. They let the regime label some expressive behavior by some persons as different. His decisions cut, first, when he decided "whether particular conduct possesses sufficient communicative elements to bring the First Amendment into play" (*Houston* v. *Hill* 1987, 2512–15; *Lakewood* v. *Plain Dealer* 1988, 2143–47; *Texas* v. *Johnson* 1989, 2539) or whether a belief or practice qualifies as religious expression (*Lyng* v. *Northwest Indian Cemetery* 1988, 1330–33, diss.). Although Brennan's acceptance of a broad generality for the amendment did not leave claimants struggling to prove that their actions were not criminal (as they would have to under Scalia's *Smith* and *Barnes* logic), claimants still had to recognize the power of the Court over their effort to lodge a claim about their rights. Second, even when the First Amendment is in play, the Court must be able to afford relief to the right's claimant. Usually Brennan implicitly made this decision when he recognized the First Amendment dimensions of a case. However, Brennan reaffirmed the power of the Court to establish or deny the opportunity for relief of their claim. The language of rules about relief thus made claimants into subjects of the power of the justices. Third and most importantly, Brennan's decisions cut between an interest in preserving order and some claimant's interests in the special protection of their expressive liberty. Because he normally used strict scrutiny analysis in free expression cases, his decisions normally favored liberty. However, his cut in favor of liberty often came with qualifications that suggested that other forms of expression might be disciplined by the state or private parties.

Several Brennan opinions provide evidence of his construction of the space for liberty and the nature of subjection. The privileged quality of liberty emerged in Brennan's consideration of public forum, free exercise, obscenity, and a few other expressive issues. His public forum decisions prohibited the state from making imprecise or speculative claims about its interests in public order on public property and the protection of national symbols to punish politically expressive flag burning (*Texas* v. *Johnson* 1989; *United States* v. *Eichman* 1990). Also, the state could not make it unlawful verbally to interrupt a police officer investigating a situation on a public street. He deemed the law to be overbroad and vague because it stifled criticism of a public official, rather narrowly serving a legitimate governmental interest in the control of disorder or words that imminently incited disorder (*Houston* v. *Hill* 1987). He wrote for the Court to overturn an ordinance that allowed mayoral regulation of the placement of newspaper dispensing machines on public property. He concluded that the ordinance afforded "unbridled discretion" to the mayor to censor the distribution of information and infringe on speech-related conduct (*Lakewood* v. *Plain Dealer* 1988). He also argued in a dissent that the state could not absolutely ban protests and demonstrations from the streets, a public forum, for the restriction was not narrowly tailored to serve a substantial but not compelling governmental interest (*Frisby* v. *Schultz* 1988, 2506–9, diss.). A narrower restriction on the use of public forums—a law prohibiting solicitations near post offices—won support from the Court, but Brennan again dissented, arguing that the space was a public forum and that the regulation of the forum was overbroad (*United States* v. *Kokinda* 1990, 3126–37, diss.).

Brennan's free exercise opinions were all dissents challenging the lower-tier-rationality standard adopted by the Court to deny free exercise claims. He sought the use of strict scrutiny and the compelling interest test even though he found that the Court often failed to satisfy its own rationality criteria (*O'Lone* v. *Shabazz* 1987, 2407–14, diss.; *Lyng* v. *Northwest Indian Cemetery* 1988, 1330–40, diss.). For example, although he did not write in the final adjudication of the Smith case, when the case initially came to the Court during the previous term he penned a dissenting opinion directly contrary to the later opinion of Scalia. Building on precedent requiring strict scrutiny of state-imposed burdens on free exercise of religion, he concluded that Oregon's contention of two purposes for the treatment of Smith did not satisfy the criteria established in precedent. He also was amazed that the majority of his Court needed to remand the case for clarification of issues when he thought the law was clear, although the Scalia opinion of the next term indicated that it was less clear than he had thought (*Employment Division* v. *Smith* 1988, 1452–55, diss.). His opinions about the regulation of obscenity also reflect a penchant for favoring claims of liberty. He dissented when the Court upheld a law against nude posing by minors because he deemed it overbroad (*Massachusetts* v. *Oakes* 1989, 2642–46, diss.). Scalia's commitment to due process rather than an attraction to the First Amendment resulted in his opposition to the same law (pp. 2639–41, conc. and diss.). Brennan dissented when the Court accepted the constitutionality of criminal penalties for the possession of child pornography because he thought that the statute was overbroad in its terminology in a way that might curtail protected expression and because it penalized the possession of expressive materials, long protected by the Court, without linking possession to the evils of the production of the material (*Osborne* v. *Ohio* 1990, 1705–15, diss.). And he dissented in part when the Court determined some aspects of dial-a-porn services to be regulable. Again, his argument was that the regulation was too sweeping and an overbroad means of securing a governmental interest (*Sable Communications* v. *FCC* 1989, 2840–41, conc. and diss.). Finally, Brennan objected to arguments that governments might use zoning and other laws, as in *FW/PBS* v. *Dallas*, to restrict the "secondary effects" of sexually oriented or other expressive activity. He countered in dissent that this argument allowed a content-based regulation of speech of an indeterminate nature likely to damage a preferred right (*Boos* v. *Barry* 1988, 1171–73, conc. and diss.).

Despite the privilege accorded claims of rights, most of Brennan's opinions qualified the space for political liberty. Some expressions remained "different" and subject to the discipline of public power. The concept of levels of scrutiny itself recognizes difference. When strict scrutiny and the concomitant compelling-state-interest test were employed, Brennan defended expressions of differences. However, the strict scrutiny test is instrumental and is justified by a pragmatic logic. If he determined that strict scrutiny was not the appropriate mode of analysis or that a compelling state interest was present, the expression of difference could be denied. For example, his arguments in the student newspaper case, obscenity cases, and the flag-burning cases recognize that some restrictions on expression such as those to provide order in public schools (*Hazelwood School District* v. *Kuhlmeier* 1988, 574–76, diss.), to protect minors from abuse (*Massachusetts* v. *Oakes* 1989, 2642–43, *diss.*; *Osborne* v. *Ohio* 1990, 1706–7, diss.), and to prevent imminent disorder or "fighting words" that provoke a breach of the peace (*Texas* v. *Johnson* 1989, 2541–42) can survive strict scrutiny. Also, his arguments in public forum cases (*Frisby* v. *Schultz* 1988, 2506–9, diss.; *Houston* v. *Hill* 1987, 2508–12; *United States* v. *Kokinda* 1990, 3134–37), the dial-a-porn and child pornography cases (*Sable Communications* v. *FCC* 1989, 2840–41, conc. and diss), and commercial speech arena (*Shapero* v. *Kentucky* Bar Association 1988, 1921) suggest that narrowly drawn statutes that serve a cognizable compelling state interest might survive to discipline certain modes of expression. Finally, as indicated by his use of a reasonableness test to evaluate the free exercise claims of prisoners (*O'Lone* v. *Shabazz* 1987, 2407–9, diss.), he will pragmatically adjust the standard of protection for liberty based upon the circumstances of the case and the need to treat the exercise of public power to check dangers that can result from differences in the status of rights claimants.

Subjection to Private Power

The expansive concept of free expression visible on the surface of Brennan's opinions constrained the use of public power to discipline people, but it also had an effect on private power. Constraints on public discipline of expression create a space for personal liberty, but personal liberty might be used to do violence to or repress the interests and aspirations of others. For example, Brennan's public forum cases make it clear that the power of private parties to regulate expression in private places remains. He expanded the notion of a public forum, but he left private space. Criminal law and some civil penalties, such as tort law and civil rights statutes, control physical violence and some manipulative repression of expression in private places but not the identification of difference and the subtle discipline of expression by parents, religions, or corporate managers. Thus, a parent might paddle a child for making a lewd gesture, a religion might excommunicate a person for expressing what the person deems a minor doctrinal difference, and a corporation might fire a salesperson for obscene language during meetings with clientele.

Also, as Brennan suggested in a religion clause case, the self-definition of a private body might infringe on the liberty of others. At least with private religious organizations, because it was "vital" to recognize the expressive autonomy of a religion, he was willing to accept this use of power even if it discriminated against persons practicing other modes of religious expression (*Corporation of Presiding Bishop* v. *Amos* 1987, 2872, conc.). The ability of a private party to discipline others with the assent of courts occurs in routine economic and family law cases, also. However, in the law of libel, the private discipline of expression has received special attention from Brennan and Scalia. Brennan opinion had sought to interpose freedom-of-expression doctrine into the common law of libel (*New York Times* v. *Sullivan* 1964). The result has been greater liberty to criticize public officials and public figures in print. During the Rehnquist Court, Brennan supported the full constitutional protection of statements of pure opinion within established First Amendment doctrine. However, he was more willing than the majority of the Court to accord protection from defamation lawsuits unless the evidence of falsehood and its harms are based upon clearly ascertainable facts (*Milkovich* v. *Lorain Journal* 1990, 2708–15, diss.). Scalia also supported some claims of expressive freedom in libel cases while a member of the Court of Appeals (*Liberty Lobby* v. *Anderson* 1984; *Ollman* v. *Evans* 1984, 1036–39, diss. in part). The result of the libel cases is not just the creation of a zone for expressive criticism in print. In these decisions, the court abets private actions to criticize others. These libel opinions directed no violence or repression at distinctive expressive groups. Yet they indicate a willingness to let private parties use the courts to police expression, and they permit private persons to use publications to label and identify others as different or normal. An author can express ideas designed to pressure conformism and to ridicule the different. With libel law (as with other rulings on expressive freedom that create liberty), subjection of individuals to the power of other private parties emerges. Ironically, freedom from the regime's power over expression can make persons the subject of private power.

CONCLUSION

The dominant normative attitudes embedded in the practice of rights in the American regime can mean the neglect of the injustices people suffer or the desecration and destruction of their values and bodies. The repression and violence against the values and bodies of persons illustrates the primary failure of the American myth of constitutional rights. Sometimes the adjudication of claims of rights does not preserve the individual liberty that is the essence of the American political project; sometimes, judges value order and write opinions that desecrate or destroy people or repress their free use of political space. In brief, the practice of rights is not a palliative for the many political pathologies of liberal regimes. Its impact is not always justice. Life remains unfair, and people suffer their world. Liberal regimes and their practices of rights do not end pain and death or even barely perceptible psychological subjection that disempowers people and diminishes their humanity. In these instances, the opinions illustrate Walter Benjamin's maxim, "Lawmaking is power making, and to that extent, an immediate manifestation of violence" (1978, 295).

Although Justice Brennan attempted to construct a practice of rights to defend against a broad array of injustices, as the discussion of expressive rights shows, his practice of rights had its limitations. His attitudes about the practice of rights still set boundaries on liberty; he used judicial power to cut. He did not free every person to fulfill and empower themselves. Less attentive to claims of liberty and enthralled with the idea of a reasoned political order constructed by judges, Justice Scalia sought outcomes that more frequently cut against claims of liberty. Reflecting this attitude, he once wrote, "This Court need not, and has no authority to, inject itself into every field of human activity where irrationality and oppression may theoretically occur, and if it tries to do so it will destroy itself" (*Cruzan* v. *Director Missouri Department of Health* 1990, 2863, conc.). His opinions, as a whole, are bent on establishing a formal law about expression that vanquishes danger, conflict, and threats to political order. Consequently, neither justice has practiced rights—or revealed an attitude about rights—that accords recognition to the liberty claims of many different expressions of political or metaphysical values.

However, while political scientists need to engage in the critique of the violence, repression, and subjection apparent in the contemporary practice of rights by the American judiciary, we must appreciate that the American practice of rights at least recognizes the value of the liberty to express differences. Nonliberal institutional structures of the state let religious, transcendental, teleological, traditional, community, collectivist, therapeutic, professional, or elite values discipline and order political space in ways that all too often restrict the space for expressive liberty. Although violence and repression or subjection to public or private power can sap the tree of liberty that the practice of rights is supposed to fertilize, the liberal practice of rights still affords some political space where the expression of some differences can occur. Also, as illustrated by the conflicting opinions of Scalia and Brennan, the practice of rights in liberal regimes leaves space for struggle about ambiguities over the boundaries of order and liberty and the propriety of disciplinary actions by the courts. If the populace assumes it has choices or is educated in the value of democracy and is willing to resist disciplinary decisions, the struggle over space for expression, the struggle for liberty, will occur. In this context, resistance to power is, to a degree, legitimate. Thus, ironically, the practice of rights in liberal regimes promotes resistance—by factions, classes, and persons engaged in everyday choices—to the boundaries on liberty that are the political function of the practice. No other political arrangement, no other design for a regime, so effectively promises criticism of itself and attention to liberty.

Notes

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1. The word *discipline* is used to describe control on political space because it implies prescription by rules more than does the use of term *coercion* (Weinstein 1972, 65). Discipline appears to be a reaction to the assumption that some people pose danger to the hegemonic control of political space by others (Dumm 1987).

2. The idea of rights as a practice views legality as the human activity of the production of categories that define and limit our discourse and ideology. Practice theory assumes that activity is rational, pragmatic, and interested and that practices can be examined through historical analysis, as I have done here. Also, besides its concern with the construction of politics and society through a practice, practice theory more generally attends to activity that reproduces or transforms the meaning of social structures (like rights) and relations of power. On practice theory generally, see Coombe 1989, 69–71, 111–21; Ortner 1984.

3. The judicial role in the practice of rights is supplemented by legislative and executive action, including the construction of statutory rights, partial control of the jurisdiction of courts over rights, and statements about rights or efforts to influence the construction of rights by participation in litigation or other activity. Private litigant choices and secondary elite and mass behavior also effect the practice of rights by judges.

4. This article discusses First Amendment conflicts about expression including freedom of speech and assembly and free exercise of religion. It does not consider questions of religious establishment, where a complex interplay of public and private violence and repression remain to be sorted out.

5. I accept the empirical conclusions of scholars like Segal and Spaeth (1993) that Supreme Court decisions are primarily a function of judicial attitudes or ideologies. My concern is that the hegemony of a few such normative attitudinal positions squeezes the expression of other attitudes out of political space.

6. That is, he determined it to be not precisely written or "narrowly tailored" so as to serve only a compelling governmental interest.

7. In a similar vein, see his Court of Appeals decision in *Block* v. *Meese* (1986).

8. The term *subjection* is from Blanchot 1987, 83–88.

9. For different interpretations of repression in Foucault's work, compare Deleuze 1988, 29, 71; Hoy 1986; Simon 1992; B. Smart 1983, 73–122; with Hunt 1992, 9–10.

10. For the argument that Foucault tries to "expel" law from modernity, see Hunt 1992, 15–20; for other readings, see Deleuze 1988, 29–30; Dumm 1990, 34–42; Simon 1992.

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AN ANALYSIS OF CONGRESSIONAL CAREER DECISIONS, 1947-1986

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Most previous research on congressional career decisions has focused on one of two binary choices—between retiring and running for reelection, or between running for reelection and seeking higher office. But most of the time, representatives face all three choices simultaneously. Employing a "mother logit" model, we estimate the effects of relevant variables both on pairwise comparisons (conditional probabilities) and on the unconditional probabilities of choosing each one of these three alternatives. Probably most intriguing is our finding that a member's age has little or no effect upon the unconditional probability of running for reelection. The interrelatedness of career options is seen particularly clearly in the case of incumbents who had been redistricted out of their seats. When they had an opportunity to run for higher office, they were likely to take it. Only when they lacked such an opportunity were they more likely than other members to opt for retirement.

Research on congressional career decisions has largely followed two separate and independent paths. The first concerns the decisions of members of the House of Representatives either to run for reelection or to retire. Many of these studies have sought to explain major shifts in retirement rates over time—the upsurge in retirements that occurred during the 1970s (Cooper and West 1981; Frantzich 1978; Hibbing 1982), their subsequent decline in the 1980s (Moore and Hibbing 1991), and the jump of retirement rates to record levels in 1992 (Groseclose and Krehbiel 1993; Jacobson and Dimock 1993). A second major area of inquiry focuses on the decision of House incumbents either to run for reelection or to abandon their seats in order to seek higher office—typically governor or U.S. senator. Adopting the terminology of Schlesinger's (1966) classic study, the latter choice is referred to as *progressive ambition.* In seeking to explain voluntary departure from the House, most previous studies of congressional career decisions have thus treated these decisions as simple binary choices: (1) retire or not and (2) seek higher office or not.

Most of the time, however, congressmen actually face all three choices—run for reelection, retire, or run for higher office—simultaneously. To examine retirements separately or progressive ambition separately artificially truncates the actual menu of choices that incumbents confront. This point is more than a conceptual or semantic one. Assume, for example, that a particular member has tired of service in the House. With an incumbent of his own party running for reelection in both the gubernatorial and senatorial races in his state, he decides to go back home and practice law. But what if the senate race were instead for an open seat? If he decided to abandon his House seat for a try at the Senate, that representative would be excluded from an analysis of voluntary retirements, because he chose neither to retire nor to run for reelection. Yet at least in this example, the member was as disenchanted of the House as any other retiree. Furthermore, when we see that at a particular point in time a member chooses to retire, we also know that they have not on some previous occasion decided to seek higher office—and vice versa.[1]

More precisely, previous studies of voluntary retirement were analyzing the conditional probability of members choosing retirement over seeking reelection, given that they had not otherwise chosen to run for higher office. Similarly, studies of progressive ambition were analyzing the conditional probability of seeking higher office versus remaining in the House, given that the member had not otherwise chosen to retire. We learn how a representative chooses from a subset of all possible alternatives but nothing about how the subset was chosen. One major consequence of not simultaneously examining all possible alternatives is a failure to specify relevant variables that enter the choice calculation. For example, previous studies of voluntary retirement have not considered the impact of key variables that affect progressive ambition, such as the desirability of another office and the likelihood of obtaining it. True, these variables probably do not affect the *conditional* probability of retiring versus seeking reelection. If they increase the probability of House members seeking higher office, however, they necessarily decrease the probability of members choosing the choice subset of {*retire, reelection*} and thus necessarily decrease the probability of retiring.

We shall formulate an integrated model of congressional career decisions, a model based upon the assumption of expected utility maximization. After a discussion of our estimation strategy, we review the two strands of research identified previously—progressive ambition and voluntary retirement—in order to identify key variables needed to enter into our model. The data used in our analysis are the career decisions made by incumbents serving in the House from the Eightieth through the Ninety-ninth Congresses (1947–86). Following the lead of previous research, we first estimate the effect of key variables upon each pairwise comparison of choices. However, we then calculate the effect of each significant independent variable upon the *unconditional* probability of choosing each of the three alternatives *run for reelection, seek higher office,* or *retire.* Our intention here is to draw as complete a picture as possible of how House members choose among their career alternatives. An important contribution of this analysis is the information it reveals about those who choose to stay in the House. Over the past several years, the percentage of incumbents defeated in their reelection bids has usually been quite low (see Figure 1). To understand the composition of the House and turnover in its membership, it has thus become particularly important to understand why and when incumbents choose to leave the House voluntarily.

AN EXPECTED-UTILITY ANALYSIS OF CONGRESSIONAL CAREER DECISIONS

We assume that representatives are rational in the sense of maximizing expected utility. Consider the choices that House incumbents face at the end of each term. Those who have an opportunity to run for a higher office have the choice set $J = \{a_1, a_2, a_3\}$, where a_1 = retire, a_2 = run for reelection to the House, and a_3 = run for a higher office. Those who do not have an opportunity for higher office choose between a_1 and a_2. Rohde (1979) considers a House member to have such an opportunity if an election is being held in the member's state for a Senate seat or for the governorship and there is no own-party incumbent seeking reelection to the office in question. Given that House members virtually never challenge an incumbent governor or senator of their own party, this would appear to be an appropriate way to proceed.

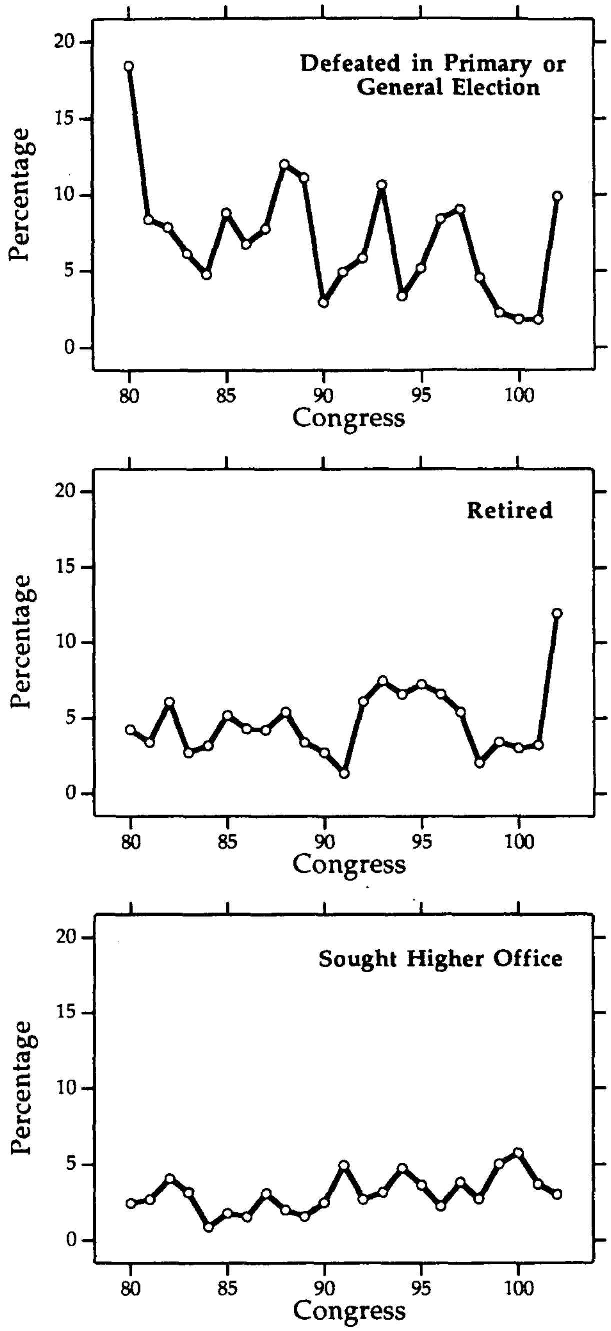
Under the rationality assumption, a representative chooses option a_j if and only if the expected utility of a_j is greater than that of all other available options. The representative who chooses to retire receives the utility of retirement U_1 with certainty, that is, probability $Q_1 = 1$. The representative who chooses to run for reelection wins reelection with probability Q_2, receiving utility from the House seat U_2, minus the cost of running for reelection C_2 and, correspondingly, loses with probability $1 - Q_2$, receiving $U_1 - C_2$. The representative who chooses to run for higher office wins with probability Q_3, receiving utility from the higher office seat U_3 minus the cost of running for it C_3, and loses with probability $1 - Q_3$, receiving $U_1 - C_3$. Let E_j denote the expected utility of choosing option a_j. Hence we have

$$\begin{aligned} E_1 &= U_1 \\ E_2 &= Q_2U_2 + (1 - Q_2)U_1 - C_2 \\ &= U_1 + Q_2(U_2 - U_1) - C_2 \\ E_3 &= Q_3U_3 + (1 - Q_3)U_1 - C_3 \\ &= U_1 + Q_3(U_3 - U_1) - C_3. \end{aligned} \tag{1}$$

A graphical illustration of the choice situation presented in equation 1 is shown in Figure 2.

FIGURE 1

Sources of House Membership Turnover

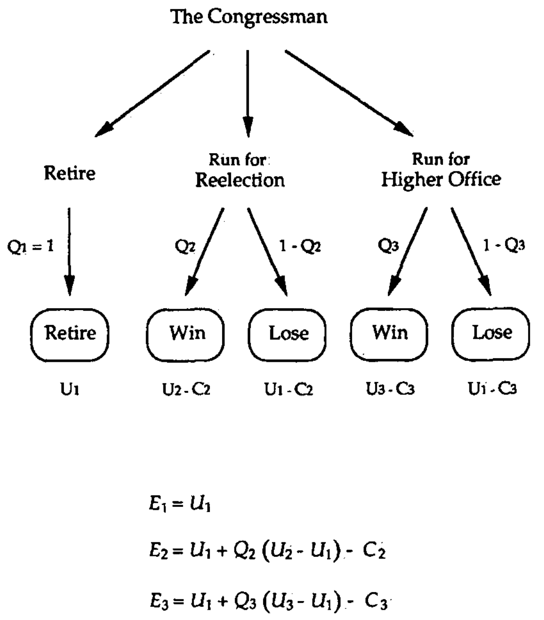


As any affine transformation of an expected utility function is also an expected utility function (Varian 1984), we can subtract U_1 from all E_j and rewrite equation 1 as

$$\begin{aligned} E_1 &= 0 \\ E_2 &= Q_2(U_2 - U_1) - C_2 \\ E_3 &= Q_3(U_3 - U_1) - C_3. \end{aligned} \tag{2}$$

FIGURE 2

Congressional Career Choices



Equation 2 states that expected utility of running for reelection depends upon the probability of winning reelection, the cost of seeking reelection, and the value of a House seat relative to the value of retirement. Similarly, the expected utility of seeking higher office depends upon the probability of winning the higher office, the cost of seeking it, and the value of the higher office relative to the value of retirement.

If we knew the values of the E_js (i.e. the expected utilities associated with each choice), we could predict with certainty which one each representative would choose. Unfortunately, their exact values are functions of many variables, which are only partially observable or measurable. The model we estimate thus necessarily includes error, and predictions of choice can be made only in probabilistic terms. Our next task is thus to select a choice model giving the probabilities of choosing to retire, running for reelection, or seeking higher office, based upon the empirical expected utilities of the alternatives.

To avoid making strong assumptions about the nature of the errors in the *E*s, we adopt what has come to be called the *mother logit* model (McFadden 1975). As formulated by Train (1986), let $P^*_{in} = f(z_{in}; z_{jn}, \forall_j \neq i; s_n)$ be the "true" probability that decision maker *n* chooses alternative *i*, where z_{in} is the observed data relating to alternative *i* faced by decision maker *n*, z_{jn} is the observed data relating to alternatives other than *i*, and s_n is a vector of characteristics associated with the decision maker. This model imposes no restrictions on the functional form of *f*. All choice models are special cases of this model, including the standard logit and probit models.

Now define $W_{in} = \log P^*_{in}$ and evaluate the logit probabilities based on W_{in}:

$$P_{in} = \frac{e^{W_{in}}}{\Sigma_j e^{W_{jn}}} = \frac{e^{\log P^*_{in}}}{\Sigma_j e^{\log P^*_{jn}}} = P^*_{in}, \tag{3}$$

where the last equality is due to the fact that choice probabilities necessarily sum to 1. Equation 3 shows that *any* choice probability P^*_{in} can be expressed in the logit probability form.[2] The next question to consider is the appropriate specification of W_i.[3] Once we obtain W_i, we can straightforwardly estimate a logit-form model. Because $W_{in} = \log P^*_{in}$ depends on the (expected) utilities of all alternatives,[4] all variables that enter the utility function of any alternative enter each W_{in}. For members facing different choice sets (i.e., with or without the option of seeking higher office), different sets of variables and coefficients may enter W_i. For those whose choice set does encompass all three alternatives, the expected utility function of equation 2 gives us

$$W_i = g_i(U_1, U_2, U_3, Q_2, Q_3, C_2, C_3).$$

Let *X* denote the vector of variables that appear in g_i and assume, for simplicity, that g_is are linear, then $W_i = \beta_i X$ where β_i is a vector of coefficients. The choice probabilities are therefore given by

$$P_i = \frac{e^{\beta_i X}}{\Sigma^3_{j=1} e^{\beta_j X}} \,. \tag{4}$$

If the choice set contains two alternatives, then U_3, Q_3, and C_3 do not enter W_i. Denoting the rest of the variables by *Y* and again assuming linearity, we have $W_i = \alpha_i Y$, where α_i is a vector of coefficients. The choice probabilities are therefore given by

$$P_i = \frac{e^{\alpha_i Y}}{\Sigma^2_{j=1} e^{\alpha_j Y}} \,. \tag{5}$$

Equation 4 can be rewritten as

$$P_1 = \frac{e^{(\beta_1 - \beta_2)X}}{e^{(\alpha_1 - \beta_2)X} + 1 + e^{(\beta_3 - \beta_2)X}}$$

or

$$P_1 = \frac{1}{1 + e^{(\beta_2 - \beta_1)X} + e^{(\beta_3 - \beta_1)X}}$$

P_2 and P_3 (and the probabilities in equation 5) can be expressed similarly. If the first alternative is taken as the base of normalization, estimation of the model will give $\beta_2 - \beta_1$ and $\beta_3 - \beta_1$. We correspondingly estimate $\beta_1 - \beta_2$ and $\beta_3 - \beta_2$ if the second alternative is taken as the base of normalization and $\beta_1 - \beta_3$ and $\beta_2 - \beta_3$ if the third alternative is the base. Normalizing utilities using different bases can facilitate the interpretation of estimation results.

In the case of individuals with only two choices available, it is easy to verify that

$$\frac{\partial P_1}{\partial Y} = P_1 P_2(\alpha_1 - \alpha_2).$$

The variables are signed by $\alpha_1 - \alpha_2$. Hence we can infer the direction of the effect upon P_1 of an independent variable y directly from the sign of the estimated coefficient of y. If there are three alternatives, then

$$P_1 = \frac{e^{\beta_1 X}}{\Sigma_{j=1}^{3} e^{\beta_j X}} = \frac{e^{\beta_1 X}}{\Sigma_{j=1}^{2} e^{\beta_j X}} \frac{\Sigma_{j=1}^{2} e^{\beta_j X}}{\Sigma_{j=1}^{3} e^{\beta_j X}},$$

where the second fraction is the probability of choosing the subset {1, 2} and the first fraction is the conditional probability of choosing alternative 1 over alternative 2 given that the subset {1, 2} is chosen. This conditional probability has the same functional form as the unconditional probability in the binary choice case, and similar analysis shows that its partial derivatives are signed by $(\beta_1 - \beta_2)$.

FACTORS INFLUENCING CONGRESSIONAL CAREER DECISIONS

In order to analyze congressional career decisions within the framework of the model just developed, we need to identify variables that enter the vector X (i.e., that affect the expected utility of competing career choices). The probability term associated with the retirement option Q_1 is of course always 1, and this option entails no costs in selecting it. The variable most frequently associated with U_1—the value of retirement—is the member's age. Without exception, previous studies in this area observe a positive relationship between age and rates of retirement. Although members who retire from Congress do not necessarily exit the labor force entirely, it is quite to be expected that advancing years induce members to give up a job that is physically, mentally, and emotionally demanding. True, we could also view age as decreasing U_2, the value of remaining in the House. Whatever the case, age should act to lower the utility of continuing to serve in the House relative to that of retirement.

Turning next to U_2, perhaps the first variable that comes to mind is the salary that members are paid. Hibbing (1982) reports some evidence that increases in congressional salaries are associated with a decline in the number of retirements, and the wide variation in salary levels among different state legislatures clearly accounts for much of the variation in the degree of careerism present in the membership (Squire 1988). Unfortunately, in the U.S. Congress salary levels are the same for all members serving at the same time, change only infrequently, and as Hibbing observes are only one component of the total compensation package. Moreover, the relevant financial calculation incumbents need to make is not salary alone but rather the difference between their congressional salary and what can be earned on the outside in the form of wages or pensions or both. For these reasons, we think it best to focus on other, nonfinancial measures.

It is also probable that some members derive more utility from serving in the House than do others, as power and influence in the House are not uniformly distributed. Most previous studies use members' seniority rankings as an indicator of their place in the House hierarchy, the hypothesis being that more senior members of the House are more powerful and thus less inclined to leave voluntarily. Unfortunately, seniority is highly correlated with age, which is hypothesized to have precisely the opposite effect upon retirement decisions. Another major problem with this variable is that what counts in Congress is not seniority rank on the floor but rather one's place in party or committee. In our view, then, it is those members who are chairmen of a standing committee or who hold a high rank in the party apparatus (i.e., Speaker, majority leader, or whip) who would be expected to place greater value on serving in the House and so be less likely to retire or to run for higher office.

A characteristic that many studies have identified as a major source of job dissatisfaction in the House is minority party status, which, since World War II, has almost always meant being a Republican. Earlier studies focused on party differences in voluntary retirement rates and report evidence of higher rates among House Republicans (Bullock 1972; Frantzich 1978). More recently, Gilmour and Rothstein (1993) and Schansberg (1992) stress that Republicans differ from Democrats in their greater propensity to seek higher office. The explanation given for these findings is that minority status implies meager influence upon policymaking and virtually no prospect of a committee or subcommittee chair.

A related hypothesis is that House conservatives, presumably due to ideological frustration, exhibit higher rates of voluntary departure. Frantzich (1978) reports some evidence for this in his analysis of congressional retirements between 1966 and 1974, but by not controlling for party membership, he may have only identified a greater propensity toward retirement of House *Republicans*. Hibbing (1982) examines this hypothesis by investigating whether or not Southern Democrats or those with high Conservative Coalition scores were more likely to decide to retire.[5] His results are generally supportive, but coefficient estimates vary a good deal from equation to equation. A potential problem with the hypotheses that minority party status and conservative ideological tendencies are a source of dissatisfaction is that of rational expectations. In other words, Republican candidates for the House must surely know that their party is unlikely to command a majority. Other conservative candidates for the House surely know that if elected, they are not going to be in the ideological mainstream of the House or even of their party. It is thus not obvious that they should subsequently find minority party status or ideological distinctiveness to be a source of frustration or job dissatisfaction.

Previous research also suggests that important political changes inside and outside of the House arena have turned what used to be a pleasant, rewarding experience into what one observer describes as the "awful empty life of a congressman" (Barnes 1988). There are many more votes (often on trivial matters), longer sessions, and increased committee work load. Keeping the job, moreover, requires more trips back and forth between Washington and the district, far more expensive campaigns, and coping with nasty, hard-to-please single-issue groups. Establishing exactly when the Golden Age of Congress ended is problematic, but most scholars point to the early 1970s, with the Subcommittee Bill of Rights, the revolt against the chairmen, and the general assault on the seniority norm seen as watershed events. To be sure, the fact that these reforms were adopted necessarily means that most House members at the time preferred them to the previous regime. Even if they were generally popular, however, the reforms could still have induced higher rates of voluntary departure among the minority of members who were injured by them. And even members who favored the reforms may have found that they made the option of leaving the House more attractive by reducing the payoffs members receive from continued service—by increasing the value of a subcommittee chairmanship vis-à-vis a committee chairmanship, for example.

Whatever the case, during the 1970s dozens of retiring representatives expressed varying degrees of resentment against these reforms, leading congressional scholars to conclude that these changes were largely responsible for the surge in voluntary retirements that took place at this time (Cooper and West 1981; Hibbing 1982). Similarly, Brace's (1984) probit analysis of progressive ambition indicates that House incumbents were more likely to abandon their seats and seek higher office after the advent of these reforms. In the equation he estimates, he tests this hypothesis by specifying a dummy variable that takes on the value of 1 for individuals serving in the Ninety-first and subsequent Congresses. A problem with this operationalization is that members elected to the House subsequent to the reforms presumably knew what they were in for, and their career decisions should therefore not be influenced by institutional changes that occurred prior to their arrival on Capitol Hill. Consistent with this reasoning is the fact that in the 1980s, the rate of voluntary retirements again declined, returning to the low levels that had been observed in the 1960s (Moore and Hibbing 1991). It is preferable, then, to posit that the effects of the House reforms should be confined to those members elected to the House prior to their implementation.

The next term we need to consider is Q_2, the probability that an incumbent will be successful in seeking reelection to the House. Previous researchers have hypothesized that members choose to leave voluntarily rather than take on another campaign entailing an unacceptably low prospect for success (Bullock 1972; Frantzich 1978). This is of course precisely what our model predicts, but the two measures that have typically been used to test this hypothesis—margin of victory in the previous election and a dummy variable indicating that a member's district had been altered due to redistricting—are not without problems. As Mann (1978) and Jacobson (1987) point out, over the past few decades a member's margin in the previous election has become a far-from-reliable predictor of performance in the next election. Bauer and Hibbing (1989) argue that Jacobson's key finding (i.e., a larger number of incumbents with previously safe electoral margins nonetheless losing the next election) is primarily due to a larger number of them being damaged by redistricting and by scandal (including Watergate). Even if an incumbent's margin of victory in the previous election is a reliable guide to future electoral prospects, however, this does not mean it is an important factor in their career decisions.

Cooper and West (1981), similarly, note that redistricting more often than not increases rather than decreases the electoral security of incumbents, and even when it is injurious the amount of damage may be inconsequential. Still, there are plenty of instances in which it is unambiguously clear that an incumbent's prospects for reelection have been substantially damaged by redistricting. This occurs when large portions of two or more districts are combined into one new district, or when a member's district is abolished completely. In such cases a member can win reelection only by running against and defeating another incumbent—a far more formidable task than defeating the weak, underfinanced challengers incumbents typically run against. Because of the abolition of at-large congressional districts and the constraints imposed by the Supreme Court's "one-person, one-vote" rulings, incumbents have become considerably more vulnerable in the redistricting process. While 18 and 21 incumbents lost their districts in the Eighty-Second and Eighty-Seventh Congresses, respectively, in the Ninety-Second and Ninety-Seventh Congresses the corresponding totals were 53 and 30.

Besides being redistricted out of a seat, another serious blow that an incumbent representative can suffer is to be caught up in a scandal. Without question, serious charges of severe misconduct have led to the resignation or retirement of dozens of representatives over the years. Previous research indicates that fairly stringent criteria should be used in identifying House incumbents who are tainted by scandal. Peters and Welch (1980) show that lesser accusations (e.g., violations of campaign finance regulations, conflicts of interest, or cronyism in hiring) have little impact on vote totals, so that most incumbents who are targets of such charges have no difficulty winning reelection. We would thus confine our attention to House incumbents who have either been criminally indicted, or who have been charged with acts of ethical or sexual misconduct that are serious enough to capture national attention. By this definition, the total count of scandals in our data is 58.

A variable that might seem to be a straightforward measure of C_2, the cost of running for reelection, is the amount of money representatives expend in their reelection bids. The problem is that most of what congressional candidates spend is other people's money, so that the real costs that candidates face are those of inducing individuals and organizations to contribute to them, as well as any political or ethical problems that flow from accepting these contributions. There is also a endogeneity problem, in that the amount of money candidates raise and spend depends upon how badly they want to keep the job. Indeed, we only observe expenditures made by candidates who have decided that the cost of running for reelection is low enough (and the value of the office is high enough) to warrant the effort. This latter problem might be finessed by lagging the campaign expenditures variable, but unfortunately previous expenditures are not all that good an indicator of current campaign expenditures. These same problems, of course, apply to campaign expenditures as a measure of the cost of seeking higher office.

The next term in the X vector to consider is U_3, the value of higher office, which, with a few exceptions, is either a governorship or a seat in the U.S. Senate. In this case, of course, the previous research that we have to draw upon comes primarily out of the literature on progressive ambition. Rohde (1979), first of all, finds that House members are much more likely to seek a seat in the Senate than a governorship. He attributes this (correctly, we think) to the fact that senators serve longer terms than governors and are more likely to win reelection.

As in the literature on voluntary retirement, much attention here has also been focused upon the importance of age. As Schlesinger puts it, "A man can fail to advance in politics as much because he is the wrong age at the wrong time as because he is in the wrong office" (1966, 174). Schlesinger reports that between 1900 and 1958, most members elected to the House for the first time were in their late thirties. Most governors took office for the first time while in their late forties, and most senators in their early fifties—considerably younger, in other words, than the age at which most members typically retire from the House. The implication of these findings is that the option of abandoning one's seat in the House for a chance at higher office would seem to be more attractive for the relatively young. Hain hypothesizes explicitly that "the older a politician is . . . the less likely he is to harbor ambitions to advance" (1974, 265).

Exactly how age enters potentially ambitious House members' utility calculations has never been well specified. Schlesinger suggests that those who enter politics at an early age are politically more ambitious, which is another way of saying that the increase in utility between their current office and the office they aspire to is especially large. In our view, a more plausible hypothesis regarding age is offered by Rohde (1979). He argues that younger members have typically served fewer terms than older members and are thus more inclined to seek a Senate seat or governorship because they have less to give up by way of power and seniority. In his probit analysis of progressive ambition among House members, Brace (1984) posits age to have a "curvilinear" effect, with both relatively young and relatively old members being less likely to pursue higher office. Because of the high degree of correlation between the age and age^2 terms needed to specify this effect, we think it is preferable to entertain the simpler hypothesis that increasing age, contrary to the role it plays in encouraging retirement, discourages House members from seeking higher office.

Previous studies of progressive ambition have been most thorough in analyzing Q_3, the probability of winning election to higher office. First, both Rohde (1979) and Brace (1984) find that House members are far more likely to run for an open seat than against an incumbent governor or senator of the opposing party. Both consider the prospect of challenging an incumbent senator or governor of one's own party as so uninviting that for all practical purposes it is no opportunity at all. That House members are more attracted to opportunities than entail a greater chance of success is further borne out by their finding that members from small states exhibit higher rates of progressive ambition than those from larger states. In the view of Rohde and Brace, this is due to the greater degree of commonality between their House district and the entire state. Rohde found this effect to be especially pronounced in states with just one or two congressional districts. We would add that large-state members contemplating a run for higher office confront a larger potential pool of formidable candidates—other House members like themselves. While the likelihood is high that a few will run anyway, many more will be discouraged.

ANALYSIS AND FINDINGS

Before estimating our mother logit model of congressional career decisions, we need to detail exactly how the variables discussed are operationalized and, where useful, briefly to reiterate our expectations as to their effects on congressional career decisions.

(1) *Age* should increase the probability of retirement to the extent that increasing age lowers the utility of remaining in office relative to that of retirement. We hypothesize age to be negatively associated with progressive ambition, primarily because younger House members have lower opportunity costs.

(2) *Leadership position* is a dummy variable that takes on the value of 1 if the member is the chair of a standing committee or a majority party leader (Speaker, majority leader, or whip).

(3) *Minority party status* is a dummy variable that

takes on the value of 1 for Republican members, 0 otherwise.

(4) *Ideological location in the party caucus,* calculated separately for Democrats and Republicans, is their percentile rank on an underlying liberal–conservative dimension derived by scaling roll-call votes with Poole and Rosenthal's (1985) NOMINATE procedure. Higher percentile rankings reflect greater conservatism.

(5) *House reforms* is a dummy variable intended to reflect the institutional and political changes that have been hypothesized to decrease the value of a seat in the House. It takes on a value of 1 for individuals serving in the Ninety-first and four subsequent Congresses (1969–78) but only if they had been initially elected prior to the Ninety-First Congress.

(6) *Previous vote* is the percentage of the total vote the member won in the previous general election.

(7) *Redistricting* is a dummy variable registering the fact that a member's district had been combined with another district or had been abolished completely, with the consequence that the member must defeat another incumbent to be reelected.

(8) *Scandal* is a dummy variable that takes on the value of 1 if House members have been criminally indicted or charged with acts of ethical or sexual misconduct serious enough or interesting enough to have warranted coverage in the *New York Times.*

Variables 1 through 5 are intended to be, in one way or another, measures of how much utility or disutility members derive from their current position in the House. Variables 6 through 8 are indicators of the probability with which members succeed in being reelected.

Four more measures are of the expected benefits of seeking a Senate seat or governorship.

(9) *Senate* is a dummy variable reflecting the greater attractiveness of a Senate seat in comparison to a governorship. It takes on a value of 1 if the member has an opportunity to run for the Senate, 0 if the only opportunity available is for a governorship.

(10) *District-to-state ratio* is the log of the reciprocal of the number of congressional districts in the member's state. We take the log in order to capture the declining marginal impact of additional districts identified by Rohde.

(11) *Open Senate seat* is a dummy variable that takes on the value of 1 if there is an open Senate seat, 0 otherwise.

(12) *Open governorship* is a dummy variable that takes on the value of 1 if the incumbent governor is not seeking reelection, 0 otherwise.

An exploratory overview of these variables is provided in Table 1, which reports mean values for members who have decided to retire, run for reelection, or seek higher office. These data derive from the choices made by incumbent members of the House of Representatives serving in the Eightieth through Ninety-Ninth Congresses. Following the coding scheme developed by Loomis (1991), those who resigned or who left the House at the end of a Congress and either quit politics or took a demonstrably lesser political position (e.g., small town mayor, city council member, small town judge, member of the state legislature, appointee to a lesser federal or state position) were considered to have retired.[6] As indicated earlier, the only higher offices we consider are Senate seats and governorships. A few members left to seek an office that was arguably superior to a House seat, such as president or vice-president, mayor of a large city, state attorney general, or state supreme court justice. The number of such cases is far too small to permit systematic analysis of them, so we simply excluded them. The only others cases we excluded were instances in which a member was expelled from the House, appointed to another office, or died in office. Finally, because the model we subsequently estimate requires that the alternatives under consideration be mutually exclusive, we adopt the decision of previous studies not to consider odd-year races for governor, since members need not surrender their House seats to compete in them.

TABLE 1

Mean Values of Factors Affecting Congressional Career Decisions: A Comparison of Members Retiring, Running for Reelection, or Seeking Higher Office

VARIABLE	RETIRING	REELECTION	HIGHER OFFICE
Age	59.6	51.4	46.6
Republican	.47	.41	.50
Chair/Leader	.09	.05	.01
Relative conservatism			
Democrat	.61	.50	.43
Republican	.54	.50	.49
Institutional reform	.21	.13	.16
Previous margin	.71	.70	.67
Scandal	.03	.01	.00
Redistricting	.03	.01	.04
Senate election	.31	.32	.76
District-to-state-ratio	.11	.12	.27
Open seat			
Senate	.11	.10	.35
Governor	.31	.30	.43
N	386	7,730	237

The entries in this table are consistent with a number of the hypotheses discussed. Those departing to seek higher office are on average much younger than retirees, and in fact are a good deal younger than the average House member seeking reelection. In keeping with the findings of Gilmour and Rothstein (1993) and Schansberg (1992), we see that nearly half of those voluntarily leaving the House are Republicans, compared to a little more than 40% of the incumbents running for reelection during this period. Also in line with their findings is the fact that Republicans appear to be even more overrepresented in the ranks of those seeking higher office than among retirees. The fact that retirees are much more likely than average to be a committee chair or party leader is no doubt a function of their age. Virtually no one gave up such a position to run for higher office. Mean values of the ideological rankings indicate that liberal Democrats are more inclined to seek higher office but that conservative Democrats exhibit a greater propensity to retire. In contrast, ideological position in the party caucus appears not to matter in the career decisions of House Republicans. Hibbing's (1982) findings are also supported by our data in that incumbent representatives who lived through the reforms of the early 1970s exhibit a greater propensity to retire—and, perhaps, a greater propensity to seek higher office as well.

Table 1 shows that the typical House incumbent won the previous election by a very large margin. The size of this margin, however, appears to have little bearing on a representative's career decisions. Being redistricted out of a seat is a rare occurrence, but those who fall victim to this outrageous fate are more likely to retire or seek higher office. Not all voluntary departures, it would seem, are all that voluntary. Being the object of a scandal is also associated with a greater propensity to retire, but we observe no instance in which a scandal-plagued incumbent abandoned his seat in the House in order to run for higher office. The entries concerning the opportunity-for-higher-office variables are all consistent with previous research. Incumbent House members find the prospect of being a senator more attractive than that of being a governor. Members of the House also seem to be able to discern a good shot at a Senate seat or governorship when they see one. Those from smaller states are considerably more likely to make a run for higher office, and running for an open seat is a much more attractive prospect than running against an incumbent of the other party.

The findings in Table 1, of course, are suggestive at best. A real test of our career decision model and the various hypotheses flowing from it requires estimation of the logit model outlined previously. As noted, many members do not have an opportunity to run for higher office, which we deem to exist only if an election is being held in the member's state for a Senate seat or for the governorship and an incumbent of the member's same party is not seeking reelection to the office in question. For this reason, we estimate separate models for those individuals having all three choices and for those whose choices are confined to retire or run for reelection. The two-choice model omits the three dummies that reflect the nature of the opportunities for higher office, which necessarily take on the value of zero for all members of this class. Keep in mind that for members without an opportunity for higher office, the estimated coefficients reflect the impact of the independent variables on the unconditional probability of choosing one option over the other. For the large majority of members who do have the option to run for higher office, the estimates refer to the *conditional* probability of choosing one option over the other, given that the third option was not chosen. For the sake of brevity and readability we do not repeatedly refer to this fact in our discussion of the analysis.

Results of the logit estimation for members with all three choices and for those with only two are reported in Tables 2 and 3, respectively. Turning first to Table 2, each pair of columns reports the coefficients and standard errors for each possible pairwise choice: $\beta_1 - \beta_2$, *retire* versus *run for reelection*; $\beta_3 - \beta_2$, *seek higher office* versus *run for reelection*; and $\beta_3 - \beta_1$, *seek higher office* versus *retire*. Note again that all independent variables are specified for each alternative. As indicated earlier, the number of House incumbents who ran for higher office in the face of a serious scandal was zero. As a result, the estimated variances explode, and for that reason we do not report coefficients for the scandal variable where the choice of seeking higher office is available.

We see from column 1 that when it comes to choosing between seeking reelection and retiring, most of the results echo the differences in mean values reported in Table 1 and thus tend to support Hibbing's (1982) hypotheses concerning voluntary retirement. Increasing age encourages members to retire, as does being involved in a serious scandal. The figures in Table 2 also indicate that House Republicans, conservative Democrats (so much for the rational expectations hypothesis), and members who experienced the institutional reforms of the 1970s were more likely to favor retirement over running for reelection. To the extent that the lower chamber serves as a farm club for the upper, the stronger tendency of liberal Democrats to run for the Senate (and to win) may contribute to what Poole and Rosenthal (1984) describe as the ideological polarization of the U.S. Senate.

The more remarkable findings in this column; however, may be the null findings—that is, the findings where effects associated with the variables are too small to reject the hypothesis that they are actually zero. Despite the problems that a low margin of victory in the previous election might portend for future electoral prospects, it appears that this does not generally persuade incumbent congressmen to go gently into that good night. Neither, surprisingly, does losing one's seat to redistricting. Nor does there appear to be any relationship between the possession of a committee chair or party leadership position and the conditional probability of retiring. The $-.15$ coefficient is in the expected direction but statistically indistinguishable from zero.[7]

This last finding suggests to us that for most members, a seat in the House is in itself so valuable that any increase in utility derived from a committee or party leadership position is in an area of their utility function where marginal returns are rapidly diminishing. This naturally calls into question the argument that Republicans are more likely to leave the House (either by retiring or by running for higher

TABLE 2

Analysis of Congressional Career Decisions, 1947–1986: Members With an Opportunity for Higher Office

VARIABLE	RETIRE VS. RUN FOR REELECTION[a]	SEEK HIGHER OFFICE VS. RUN FOR REELECTION[b]	SEEK HIGHER OFFICE VS. RETIRE[c]
Constant	−8.52**	1.20	9.73**
	(.81)	(.67)	(1.04)
Age	0.77**	−.04**	−.12**
	(.007)	(.008)	(.01)
Republican	.89**	−.49	−1.38**
	(.32)	(.28)	(.42)
Chair/leader	−.15	−.87	−.72
	(.28)	(.73)	(.78)
Relative conservatism			
Democrat	1.40**	−1.23**	−2.63**
	(.39)	(.39)	(.55)
Republican	.05	.18	.12
	(.35)	(.35)	(.49)
Institutional reform	.37*	.42*	.05
	(.18)	(.20)	(.26)
Previous margin	−.01	−.001	(.008)
	(.005)	(.004)	(.007)
Scandal	2.05**	—	—
	(.44)		
Redistricting	.16	1.75**	1.59*
	(.53)	(.38)	(.63)
District-to-state ratio	−.11	.94**	1.05**
	(.09)	(.095)	(.12)
Senate election	−.11	1.10**	1.21**
	(.17)	(.20)	(.26)
Open Seat			
Senate	.21	.82**	.61*
	(.20)	(.16)	(.26)
Governor	−.18	.35*	.53*
	(.15)	(.15)	(.21)

Note: Only members with an opportunity for higher office are included. Standard errors are in parentheses. Initial log-likelihood is −5,407.4; at convergence, −1,573.4. N = 4,922.

[a] $\beta_1 - \beta_2$.

[b] $\beta_3 - \beta_2$.

[c] $\beta_3 - \beta_1$.

*Significant at .05 level.

**Significant at .01 level.

office) because they are frozen out of leadership positions. Perhaps it is also the case that the rewards of holding such positions, especially for older members, must be weighed against the costs of a heavier-than-normal work load and set of responsibilities. Presumably, an aging member could always choose to resign a committee chair while remaining in the House (a sort of chairman emeritus), but we are aware of no one who has done so voluntarily. Whatever the case, this finding runs contrary to the popular image of geriatric committee chairmen hanging around, long into their dotage, to retain their coveted prize as long as possible. Perhaps congressional observers have fallen prey to something of a perceptual bias. Because it takes many terms of service in the House to advance up the seniority ladder and become chairman of a committee, most committee chairmen are pretty old. But this does not mean that most old members are chairmen. And once age, party, and several other factors are taken into account, possession of a committee or party leadership position does not appear to encourage members to put off retirement any longer than they would have otherwise.[8]

Turning to the coefficients in column 2 showing effects on the choice between running for reelection versus seeking higher office, we see that Rohde's findings concerning the structure of opportunities for higher office are strongly supported. Members from smaller states are considerably more likely to make a run for higher office. Running for an open seat is a much more attractive prospect for House members than running against an incumbent governor or Senator of the other party, and the large size of the Senate election dummy also coincides with Rohde's finding that members of the House are much more likely to be attracted by a shot at the Senate than by a governorship. Although members who are redistricted out of their seats appear no more likely to choose retirement, they are more likely to seek higher office.[9] Older members are relatively less likely to seek a Senate seat or governorship, while liberal Democrats and those who were in office during the reforms of the early 1970s are more likely. The sign of the coefficient associated with the chair/leader dummy is consistent with the hypothesis that House leaders are less likely to seek higher office, but the coefficient does not achieve conventional levels of statistical significance.

These findings are thus broadly consistent with the patterns observed in Table 1 and in previous research. This is not true of our findings concerning House Republicans. Our estimates indicate that they were no more likely than Democratic members to seek higher office rather than run for reelection. (The negative sign of the coefficient is actually in the opposite direction of that predicted.) This is contrary to what Gilmour and Rothstein (1993) observe, namely, that the higher rate of Republican departures from the House is almost entirely due to greater progressive ambition. One possible reason for this discrepancy is that our analysis extends much farther back into time than theirs. Specifically, our data series includes a period (the late 1940s and early 1950s) when the Republicans actually controlled the House for a couple of Congresses and so had not yet been relegated to "permanent minority" status. We therefore modified the Republican party dummy variable to take on the value of 1 only after the 1958 election, figuring that it was about then that the Republicans began abandoning all realistic hope of retaking the House in the foreseeable future. This change moved the estimated coefficient in the predicted (positive direction), but only by a small amount.

We therefore think that the major reason for the difference between our findings and those of Gilmour and Rothstein is due not to differences in our data series but rather to the fact that we control for a broad range of other variables in which there are differences between Republican and Democratic House members. First, it should be remembered that the zero-order comparisons in Table 1, which are based upon our entire data series but do not control for these other factors, echo their finding of greater progressive ambition among Republicans. We also find that during the period of our study Republicans were about 10% less likely than Democrats to be blocked by an incumbent governor or senator of their own party. Republican members also tended to come from states with smaller congressional delegations and were somewhat more likely to be redistricted out of their seat. All these factors would predispose Republicans to higher rates of progressive ambition. We think that age differences also worked in this direction, but here, the difference in data sets does matter. Although there is no discernible difference between the average age of Republican and Democratic members in our data series, during the time frame of Gilmour and Rothstein's study Republicans were nearly two years younger on average than Democrats.

As indicated earlier, studies of congressional career decisions typically focus on one of two binary choices—*retire* versus *run for reelection* or *seek higher office* versus *run for reelection*. In an analysis of "congressional quits," Schansberg (1992) examines the third possible choice pair, *retire* versus *seek higher office*. Our analysis of the conditional probability of choosing to seek higher office versus retire is reported in Table 2, column 3. The coefficients and standard errors were obtained by estimating the model with the first alternative, retirement, as the base of normalization. It can be seen that each coefficient here necessarily equals the coefficient in column 2 minus the corresponding coefficient in column 1. We see here that the probability of choosing to retire rather than seek higher office was dramatically higher for older members, for Republicans, and for conservative Democrats. In contrast, favorable values on the opportunity variables (district-to-state ratio, Senate election, and the two open-seat variables) predisposed members to make the opposite choice.

A possibility that occurred to us while undertaking this analysis was that congressional career decisions might be modeled more accurately by a tree structure. In our introduction, we described the hypothetical situation of a member who, having decided to leave the House, was choosing between running for the Senate or getting out of politics entirely. Alternatively, members may first decide whether to run for higher office or not and if not, then decide whether to seek reelection or to retire. If decisions follow such a tree structure (i.e., one prior choice followed by a choice between two "similar" alternatives), then a model that specifically allows for correlation between the error terms of the two alternatives (e.g., a nested logit model) may be appropriate. We estimated such models but found that the similarity parameters were outside of the admissible range for proper probability choice models.

TABLE 3

Analysis of Congressional Career Decisions, 1947–1986: Members With No Opportunity for Higher Office (Retire vs. Run for Reelection)

VARIABLE	COEFFICIENT ($\alpha 1 - \alpha 2$)	S.E.
Constant	−9.01**	.89
Age	.07**	.01
Republican	1.01**	.39
Chair/leader	−.23	.29
Relative conservatism		
Democrat	1.54**	.43
Republican	.40	.44
Institutional reform	.21	.21
Previous margin	−.001	−.01
Scandal	1.90**	.61
Redistricting	1.54**	.44
District-to-state-ratio	−.01	.1

Note: Only members with no opportunity for higher office are included. Choice is between retiring and running for reelection. Initial log-likelihood is −2,378.2, at convergence, −607.3. $N = 3{,}431$.
*Significant at .05 level.
**Significant at .01 level.

The results of our logit analysis for House members whose career choices were confined to either running for reelection or retiring are reported in Table 3. For the most part, the coefficients reported here are indistinguishable from the coefficients in the corresponding $\beta_1 - \beta_2$ column in Table 2.[10] There is one crucial exception, namely, the case of the redistricting variable. In contrast to the tiny .16 coefficient in Table 2, the 1.54 coefficient reported here is quite large and statistically significant. We see, then, that House incumbents who have been redistricted out of their seats are far more likely than other House members to seek higher office when that option is available but no more likely to retire. Retirement is chosen with a much higher probability than usual, however, when the opportunity to seek higher office does not exist. In our view, this finding illustrates better than perhaps any other the importance of analyzing all congressional career options simultaneously.

CALCULATION OF UNCONDITIONAL CHOICE PROBABILITIES

Although some of the findings reported in Table 2 are new, the analysis reported there does not take us much beyond previous research on voluntary retirement and on progressive ambition. As in prior studies, the coefficients register only the effect of the independent variables upon conditional choice probabilities in the various pairwise comparisons. As we have argued, however, a deeper understanding of congressional career decisions requires us to go beyond the separate consideration of binary choices. The real value of the logit analyses we have undertaken is that the results allow us to estimate effects of key variables upon the *unconditional* probability of selecting among the three options *retire, run for reelection,* and *seek higher office.* To do this, we need to compute the elasticities of the choice probabilities. This is the percentage change in the probability of selecting a particular choice (holding other variables constant), given a percentage change in an independent variable—that is,

$$\frac{\partial \log P}{\partial \log P} = \frac{\partial P}{\partial x}\frac{x}{P}.$$

For dummy variables, we need to calculate the percentage change in choice probabilities with respect to the change from 0 to 1, holding other variables constant. The average effects on choice probabilities of all independent variables with statistically significant coefficients in Table 2 are reported in Table 4. (These were computed only for members with all three choices available.)

Note that proper interpretation of the elasticities requires taking into account the typical probabilities associated with each choice and the range of the independent variables. For example, the 3.89 elasticity of P_3 with respect to a change in the redistricting dummy from 0 to 1 implies a 389% increase in the probability of seeking higher office, say from a typical value of 3% to over 14%. In contrast, the −.15 elasticity of P_2 with respect to the same variable implies that being redistricted out of their seat decreases the probability of incumbents running for reelection from perhaps 92% down to 78%.

Table 4, row 1 (pertaining to the age variable) clearly illustrates the difference this makes in the nature of the inferences we draw from the data on congressional career decisions. Recall from Table 2 that the younger the members, the higher the conditional probability that they choose to run for reelection versus retire. But this does not mean that they have a significantly higher *unconditional* probability of remaining in the House. As we also know from Table 2, younger members were also more likely to abandon the House in order to seek higher office. The resultant elasticity of P_2 (choosing to run for reelection) with respect to age of −.09 implies that a 20% increase in a member's age (e.g., from 50 to 60) lowers their probability of seeking reelection by a negligible 1.8% (e.g., from 94% to 92.3%). Given the strong effect of age upon the conditional probability of retiring, voters in congressional elections might be persuaded to vote for younger candidates in the belief that they would have a greater likelihood of eventually becoming a party or committee leader. That age has little effect upon the unconditional probability of choosing to run for reelection, however, implies that it makes little sense for voters to practice this sort of age discrimination.

TABLE 4

Effects of Key Independent Variables Upon Unconditional Choice Probabilities

VARIABLE	RETIRING	REELECTION	HIGHER OFFICE
Age	3.70	−.09	−2.52
Republican	1.37	−.04	−.04
Relative conservatism (D)	.51	−.01	−.36
Institutional reform	.40	−.04	.45
Scandal	5.12	−.20	−.20
Redistricting	.01	−.15	3.89
District-to-state ratio	−.04	−.04	.88
Senate election	−.05	−.05	2.54
Open seat			
Senate	−.06	−.06	1.11
Governor	−.01	−.01	.39

Note: For the age, relative conservatism, and district-to-state ratio variables, the entry reported in this table is the percentage change in the unconditional probability of selecting a particular choice (holding other variables constant) given a percentage change in the independent variable. For the rest of the variables, which are all dummies, the entry is the percentage change in the unconditional probability of selecting a particular choice with respect to the change from zero to one in the independent variable. $N = 4{,}922$.

The effects of a member's ideological position in the Democratic Caucus operate in a similar manner. As indicated in Table 2, conservative Democrats are more likely than liberal Democrats (all other things constant) to choose to retire rather than run for reelection. But because liberal Democrats are more likely to run for higher office, the effect of ideological position upon the unconditional probability of running for reelection is practically zero.

As it turns out, the only variables that do have a dramatic (negative) effect on the unconditional probability of choosing to run for reelection are the scandal and redistricting variables. While redistricting dramatically increases the probability of choosing to seek higher office, the major impact of scandal is to make retirement a much more attractive option. The figures in Table 4 also confirm that Republicans differ from Democrats primarily in their greater propensity to choose retirement, and are only slightly less likely to run for either reelection or higher office. Continuing down the table, we see that favorable values of the variables registering the nature of the opportunities for higher office obviously exert a strong impact upon P_3. Interestingly, the effects of the variables reflecting the nature of available opportunities for higher office, though small, are virtually identical for P_1 and P_2. We saw in Table 2 that these opportunity variables did not affect the *conditional* probability of choosing to retire versus running for reelection to the House. It is hard to see why they should. To the extent that such variables increase the probability of House members seeking higher office, however, they necessarily decrease the probability of choosing the subset of {*retire, reelection*} and thus necessarily decrease the probability of retiring.

CONCLUSION

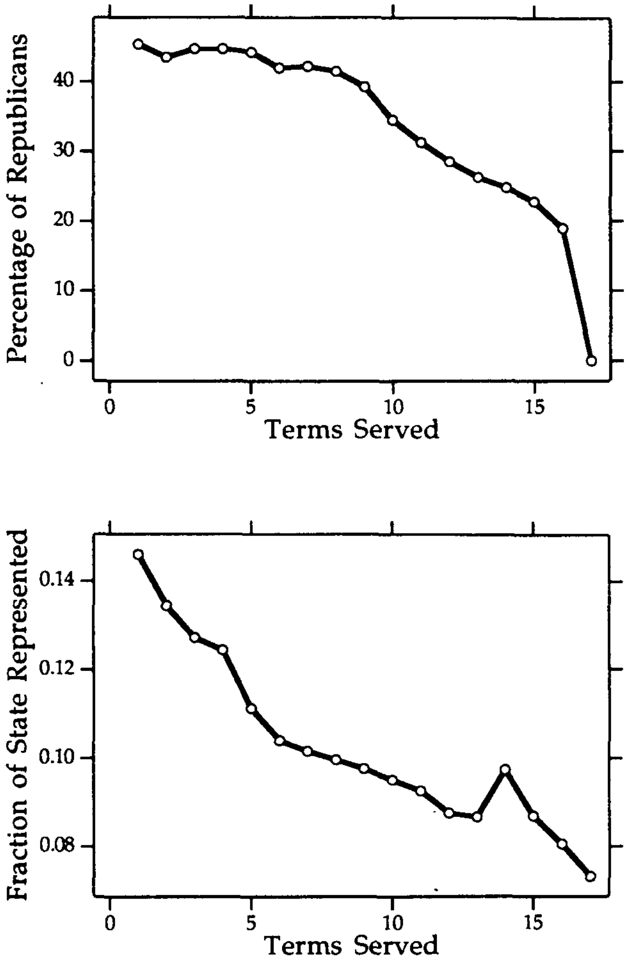
Many of the results produced in our analysis support those of previous researchers. In the case of the choice between retiring and running for reelection, for example, we find clear support for Hibbing's (1982) hypotheses about the greater likelihood of conservative Democrats, Republicans, and members who had experienced the reforms of the early 1970s to choose retirement. We also find consistent support for Rohde's (1979) hypotheses about the effect of the opportunity structure confronting House incumbents upon their probability of choosing to run for higher office, rather than for reelection to the House. However, our results, especially those concerning the unconditional probability of members choosing either to run for reelection, seek higher office, or retire, shed some new light onto the nature of congressional career decisions. Notably, we find that knowing the age of a member of the House tells you virtually nothing about the likelihood that he or she will run for reelection, that formal committee and party leadership positions and previous vote margins do not appear to figure into House members' career decisions, and that House Republicans exhibit no higher a level of progressive ambition than do Democrats. Finally, the interrelatedness of congressional career options is seen particularly clearly in the decisions made by those incumbents who had the dubious distinction of being redistricted out of their seats. When they had an opportunity to run for higher office, they were inclined to take it. Only when they lacked such an opportunity were they more likely than other members to opt for retirement.

We would like to conclude with a number of observations concerning the implications of our findings and to point to what we think are some potentially fruitful avenues for future research. The first issue we would raise is an econometric one. In our data, individual members are sampled every Congress in which they serve in the House. It is therefore likely that some degree of autocorrelation in the errors of the expected utility functions is present. The fact that the cross-sectional sample size is far larger than the number of repetitions (the data set includes nearly two thousand different individuals, who on average serve only about five terms) probably serves to alleviate this problem but does not eliminate it. We expect that the performance of the model would be improved if information about the error structure were explicitly incorporated. Developing a theoretically sound model that does this and that remains computationally feasible is a high priority for future research.

As indicated earlier, in an era in which very few incumbents stumble at the polls, it is the joint effects of progressive ambition and voluntary retirement that largely determine the composition of the House of Representatives. In understanding the impact of these career decisions upon the nature of the House membership, it should also be realized that the effects of these decisions are cumulative. Figure 3 illustrates the impact that differential patterns of voluntary departure have had upon the seniority structure of the House. In the ranks of the most senior House members—the careerists, as Bullock calls them—Republicans and members from small states are few and far between. What difference does this make? Plenty, we think. Even in the House of today, more senior members are more effective legislators. They enjoy higher rank on committee, have more legislative experience, more policy expertise, more "chits" collected from past bargains made with fellow legislators, and more and better contacts in the executive branch and in the agencies. Districts in small states or those that have elected a Republican are more likely to be represented by a relatively junior incumbent, lacking, to some degree, in both experience and connections.

FIGURE 3

Individual Career Choice and House Membership Composition



Finally, it is probably incumbent upon us to say something about the record number of voluntary departures from the House in 1992. There is at least one new wrinkle present. As Moore and Hibbing

(1991) observe, 1992 was the last year that incumbents could convert leftover campaign funds into cash; according to their figures, 13 of the incumbents announcing their retirement this year had in excess of two hundred thousand dollars of convertible campaign funds. Groseclose and Krehbiel (1993) confirm that this "golden parachute" induced a large number of retirements. The factors identified in our model, however, account fairly well for the 1992 surge in retirement. Although not all of them were serious enough to satisfy our definition of a scandal, the furor involving their overdrafts at the House bank undoubtedly encouraged several other members to opt out (Jacobson and Dimock 1993). Nineteen Ninety-Two also followed a decennial census year, so the number of "voluntary" departures was boosted even more by the inevitable casualties of redistricting. Indeed, Katz cites redistricting as the "single biggest reason that more members are retiring now than in previous years (1992, 851). Finally, we agree with Cover (1983) that it is best not to make too much of sudden shifts in the number of members voluntarily departing the House. What goes down must eventually come back up. After a number of Congresses with lower-than-usual amounts of turnover, the resultant increase in the "fleet age" of the membership necessarily portends a higher-than-usual number of retirements somewhere down the line. The last three Congresses, of course, had witnessed quite low numbers of both voluntary departures and defeats.

Notes

An earlier version of this paper was presented at the 1992 annual meeting of the Midwestern Political Science Association, Chicago. We would like to thank Keith Poole, Michael Loomis, and Jonathan Katz for their generous assistance and Paul Brace, John Gilmour, David Grether, John Hibbing, Morgan Kousser, Forrest D. Nelson, Peter Ordeshook, Paul Rothstein, Tom Palfrey, and Tom Schwartz for their comments and criticisms.

1. It should be noted that there have recently appeared a couple of studies that look at both voluntary retirement and progressive ambition, instead of only one or the other (Gilmour and Rothstein 1993; Schansberg 1992).

2. Note that the mother logit model is very general. In contrast, the standard logit model with a probability expression like equation 3 assumes that the underlying (expected) utility for alternative i is $W_{in} + \epsilon_{in}$, where ϵ_{in} is the stochastic part of the total utility following a type 1 extreme value distribution and that the decision maker acts so as to maximize his or her expected utility. In practical terms, the major difference between a standard logit model and a mother logit model thus lies in the interpretation and specification of W_{in}. In a logit model, it is the expected utility of alternative i to individual n and need not depend on attributes not related to alternative i. In a mother logit model, however, $W_{in} = \log P^*_{in}$ generally depends not only on observed data relating to alternative i but also on data relating to all other alternatives. The mother logit model, therefore, need not exhibit the independence-from-irrelevant alternatives property, in contrast to the standard logit model (Train 1986, 22).

We should note, however, that the desirable generality of the mother logit model is accompanied by difficulty in operationalizing the W_{in}s. Lacking information about the functional forms of the true probabilities, functional forms of W_{in}s are basically unknown and are subject to arbitrary assumptions. In this study, for example, we assume that they are linear in parameters in order to simplify computation. The correctness of the model depends, to a large extent, on the correctness of this assumption. We should point out that similar problems also exist in the standard logit model. There we know that W_{in}s are utilities, but in practice we rarely know the functional form of these utility functions. As here, linearity is usually assumed without much substantive justification.

3. For notational simplicity, we hereafter drop the subscript n in W_{in}.

4. See n. 2.

5. For many years it appeared to congressional scholars that retirement rates were particularly low among southern Democrats. Bullock (1972) shows that from 1941 through 1970 the percentage of southern Democrats who were "careerists" (present for 10 or more terms) was considerably higher than the percentage among northern Democrats. According to Kiewiet and McCubbins (1991), however, this difference between northerners and southerners was entirely due to the election of 1946, in which large numbers of northern Democrats were defeated but very few southerners. They appear to regard this finding as a startling new discovery, but it turns out that Wolfinger and Heifitz Hollinger (1970) made the same observation many years ago.

6. After obtaining Loomis's data on career decisions, we checked it against other data provided to us by Jonathan Katz and by Keith Poole. We resolved any discrepancies, which turned out to be very few in number, by consulting relevant issues of the *Congressional Quarterly Weekly Report*, *Congressional Quarterly Almanac*, and the *Biographical Directory of the United States Congress*.

7. We also investigated the possibility that a simple dummy for committee chairs is too crude a measure, primarily because it ignores the varying expectations that members might have regarding the prospects of their eventually attaining a chairmanship. We therefore created a variable for each House Democrat which, on the basis of life-expectancy tables, registered the probability that they would outlive all other higher-ranking Democrats on their committee. In the case of members with more than one committee assignment, the highest probability was used. When this variable was substituted for the chair/leader dummy in the equations we estimated, the coefficients remained small and insignificant.

8. One possible explanation for this finding that occurred to us was that chairmen or party leaders might hold on to their positions until they died in office and thus exit the sample without ever retiring. We found, however, that death rates were no higher for committee chairmen and party leaders than for ordinary members.

9. The redistricting variable may be subject to a bit of endogeneity, in that those drawing the lines may have less compunction about eliminating the district of an incumbent House member if it is known in advance that that member is planning to retire or to run for higher office. When asked how the state would be redistricted after losing three seats in in the 1960 round of reapportionment, Stephen McCann, Democratic floor leader in the Pennsylvania House, answered, "Show me three congressmen who are willing to give up their seats" (Cooke and Keefe 1962, 150). McCann was unfortunately unable to find three such members. Since the court rulings of the 1960s, of course, the constraint that districts must contain virtually identical numbers of people makes it more difficult to protect all incumbents in a state or portion of a state that has lost population since the last census.

10. The similarities between $\alpha_1 - \alpha_2$ and $\beta_1 - \beta_2$ prompted us to estimate a model with pooled samples. This model was indistinguishable from the current model in terms of goodness-of-fit criteria.

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RESEARCH NOTES

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GOVERNMENT PARTISANSHIP, LABOR ORGANIZATION, AND MACROECONOMIC PERFORMANCE: A CORRIGENDUM

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Alvarez, Garrett and Lange (1991) used cross-national panel data on the Organization for Economic Coordination and Development nations to show that countries with left governments and encompassing labor movements enjoyed superior economic performance. Here we show that the standard errors reported in that article are incorrect. Reestimation of the model using ordinary least squares and robust standard errors upholds the major finding of Alvarez, Garrett and Lange, regarding the political and institutional causes of economic growth but leaves the findings for unemployment and inflation open to question. We show that the model used by Alvarez, Garrett and Lange, feasible generalized least squares, cannot produce standard errors when the number of countries analyzed exceeds the length of the time period under analysis. Also, we argue that ordinary least squares with robust standard errors is superior to feasible generalized least squares for typical cross-national panel studies.

In a study of cross-validation using a model and data from Alvarez, Garrett and Lange (1991) (hereinafter AGL), Beck and Katz (1992) revealed a problem with the earlier statistical analysis. The problem is that limited data makes the procedure used by AGL (particularly for the standard errors) incorrect.[1] Here we present a new method that allows for the computation of the basic AGL model, including standard errors. We find that the major conclusion of AGL—concerning the political and organizational determinants of *economic growth*—still holds but their conclusions regarding similar determinants of inflation and unemployment are open to question. Given the problems involved in the AGL analysis and the growing use of panel data in the study of comparative politics (e.g., Blais, Blake, and Dion 1993; Hicks and Swank 1992; Swank 1992), we feel it is important to discuss some methodological issues in the analysis of cross-national panel data.

AGL estimated a model relating political and labor organization variables (and some economic controls) to economic growth, unemployment, and inflation (building on Lange and Garrett 1985). The argument was that economic performance in advanced industrial societies was superior when labor was both encompassing and had political power or when labor was weak both in politics and in the market. A mismatch of labor's organizational and political strength, on the other hand, led to inferior economic performance. AGL estimated a model where economic performance was determined by

$$E(\text{Economic Performance}) =$$

$$\beta_0 + \beta_1\text{LORG} + \beta_2\text{LFCAB} + \beta_3\text{LORG} \times \text{LFCAB}$$

$$+ \gamma\text{Vulnerability to the International Economy}$$

$$+ \rho\text{Lagged Economic Performance} + \varepsilon. \qquad (1)$$

They undertook separate analyses for three measures of economic performance: the growth of gross domestic product, the change in unemployment, and the change in the rate of inflation.[2] The independent variables of most interest are labor organizational strength (LORG, a time-invariant multiplicative index of the density and centralization of trade unions) and percentage of left-held cabinet positions (LFCAB) as a measure of labor political strength. The value of β_3, which measures the interactive impact of LORG and LFCAB on economic performance, provides the critical test of AGL's theory. The economic controls—vulnerability to the international economy—are world economic performance measures reflecting the openness of Organization for Economic Coordination and Development (OECD) economies; the lagged performance variable reflects the dynamic nature of the model.[3]

AGL used panel (time series cross-section) data to estimate equation 1, with data for 16 OECD nations over the period 1970–84, producing 15 complete observations per country.[4] We specified the variance–covariance matrix of the "error" process using a model of Parks (1967) as discussed by Kmenta (1986, 622–25). This model allowed for errors to be (1) serially correlated (in AGL this is country-specific), (2) heteroscedastic (country-specific and time-independent), and (3) contemporaneously correlated (across countries).

The Parks method uses feasible generalized least squares (FGLS) for dealing with this error process. This requires estimating the covariance matrix of the country errors and then transforming the data to remove this covariance. However, the estimated covariance matrix is singular if, as in AGL, the number of time points (T) is less than the number of cross-sectional units (N) in the panel. This singularity makes application of feasible generalized least squares impossible.

To see the problem with the Parks method, assume that the data present no serial correlation. Let t subscripts refer to years, running from 1 to T; let i and j subscripts refer to countries, running from 1 to N. Let $\mathbf{\Omega}$ be the time-independent contemporaneous covariance matrix of the errors, with ω_{ij} the contemporaneous correlation for countries i and j and with ω_{ii} being country-specific but time-independent, heteroscedasticity. We assume the data is ordered so that the first N data points refer to the countries at time 1 and so forth. The variance–covariance matrix for all observations is then $\mathbf{\Omega} \otimes \mathbf{I}_T$, where $\otimes$ is the Kronecker product and $\mathbf{I}_T$ is a T × T identity matrix. $\mathbf{\Omega}$ is estimated using the residuals from an ordinary least squares (OLS) estimation of equation 1, that is,

$$\hat{\omega}_{ij} = \frac{\Sigma_{t=1}^{T} e_{it} e_{jt}}{T}.$$

If E is the T × N matrix of residuals, then

$$\hat{\mathbf{\Omega}} = \frac{\mathbf{E}'\mathbf{E}}{T}.$$

That this matrix be nonsingular is required by FGLS. Unfortunately, $\hat{\mathbf{\Omega}}$ is singular if $T < N$.[5]

Here we offer a simpler technique than that used by AGL, which yields consistent (but inefficient) estimates of the coefficients of equation 1. Most important, however, it yields consistent estimates of the standard errors, which allows us to draw inferences about the coefficient estimates.

Let us assume that whatever serial correlation exists is constant across countries. The Parks model, on the other hand, allows for the errors to show country-specific serial correlation. It is hard to see why the parameters of equation 1 should be constant across countries while the "nuisance" serial correlation parameters should vary from country to country. As a practical matter, it is also difficult to estimate a serial correlation when we have so few time points; the estimates of country-specific serial correlations with the AGL data are very unstable.

Once we assume a single serial correlation process, it is easy to test whether the errors show serial correlation. A Lagrange multiplier test of the hypothesis that this single serial correlation is zero cannot be rejected at any conventional significance level; moreover, the estimated serial correlation is substantively small.[6]

In the presence of lagged dependent variables, OLS is consistent if the errors are temporally independent. Thus we no longer need to use the instrumental variable technique used by AGL. However, OLS will still be inefficient in the presence of contemporaneously correlated errors and heteroscedasticity.[7] AGL dealt with this via FGLS, which is the cause of the incorrect standard errors. We instead include a dummy variable to mark each year (save one). This picks up anything special about the economy in, say, 1973, and accounts for the bulk of country-pair-specific error correlation. However, this method does not account for all country-pair-specific correlation, since there may be special ties between, say, Germany and Austria, that go beyond the general year effect.

The OLS standard errors will be inconsistent in the presence of country-specific heteroscedasticity or country-pair-specific contemporaneous correlation of the errors. Analysis shows that both these phenomena persist in the data, although not very strongly. An obvious solution would be to create country-specific dummy variables, but this is not possible here because LORG does not vary across time.

We thus calculate consistently estimated ("robust") standard errors, as well as the more usual OLS estimates of the standard errors, for equation 1. These robust standard errors are estimated using the general method suggested in White (1984).[8] This method does not change the coefficient estimates but does improve estimation of standard errors. If **X** is the data matrix, the robust variance–covariance matrix of the errors is estimated by

$$(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}' \frac{(\mathbf{E}'\mathbf{E})}{T} \otimes \mathbf{I}_T\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}.$$

The results of the reanalysis can be found in Table 1. Using either the OLS or robust standard errors, we see that AGL's substantive finding on the interactive effect of labor and left government on economic growth holds. The interactive coefficient in equation 1, β_3, is positive and about four times its estimated standard error, while the estimates of β_1 and β_2 are both negative and between two and three times their estimated standard errors. Left government alone or an encompassing labor movement alone hurt economic growth, but in combination, they clearly enhance economic growth. However, AGL's results for unemployment and inflation do not fare as well. The interaction terms have the predicted signs, but neither is close to being statistically significant. Since the rate of economic growth is probably the best single indicator of national well-being and since economic growth was the source of the controversy engendered by Lange and Garrett (1985), it is thus most important for the conclusions of AGL that their result for economic growth holds.

There is little difference between the OLS and robust standard errors in Table 1. This is an indication that the AGL data do not show much contemporaneous correlation of the errors once the yearly dummy variables are introduced. With large samples, it is likely that the robust standard errors will never be inferior to OLS standard errors, and with severe contemporaneous correlation, the robust standard errors are more accurate than the OLS standard

TABLE 1

Ordinary Least Squares Estimates for Pooled Model

INDEPENDENT VARIABLE[a]	GROWTH	INFLATION[b]	UNEMPLOYMENT[b]
Constant	4.65	.32	.38
SE[c]	(.66)	(.66)	(.23)
robust SE[d]	(.65)	(.35)	(.11)
Dep. var. (lagged)	.077	−.136	.301
SE	(.066)	(.068)	(.070)
robust SE	(.094)	(.111)	(.11)
Vulnerability to OECD demand	−.0025	.001	.001
SE	(.0018)	(.002)	(.0008)
robust SE	(.0019)	(.002)	(.001)
OECD export prod.	.002	−.004	.0005
SE	(.0011)	(.001)	(.0004)
robust SE	(.0012)	(.001)	(.0004)
OECD import prod.	−.0009	.003	−.002
SE	(.0016)	(.004)	(.001)
robust SE	(.0017)	(.004)	(.001)
Labor organization	−.700	.051	−.019
SE	(.216)	(.156)	(.05)
robust SE	(.280)	(.143)	(.046)
Left cabinet	−.023	.012	.0005
SE	(.009)	(.023)	(.0004)
robust SE	(.007)	(.023)	(.007)
Interaction	.012	−.010	−.0007
SE	(.003)	(.009)	(.0029)
robust SE	(.003)	(.008)	(.003)

Source: Data on 16 OECD nations, 1971–84 from Alvarez, Garrett, and Lange (1991).
Note: Main entries are unstandardized OLS coefficient estimates. Estimates are computed with RATS 4.02.
[a] Regressions include a series of yearly dummy variables, not shown.
[b] All variables are in first differences.
[c] OLS standard errors.
[d] Robust standard errors, based on White (1984).

errors. Since the robust standard errors are easy to compute and cross-national panel studies usually have large sample sizes, we would recommend that cross-national panel analysts who use OLS also compute robust standard errors.

Finally, we would like to emphasize that the problems with the AGL methodology cannot be avoided by simply adding a few extra observations per cross section. Most cross-national panel studies do have more time points than countries; accordingly, they may be estimated by FGLS. But the use of FGLS may lead to severe underestimates of standard errors in many cross-national panel studies, which leads us to recommend that analysts consider OLS with robust standard errors unless T is much greater than N.

If T is slightly larger than N, the estimated $\hat{\Omega}$ matrix is no longer singular but contains about half as many ($N(N-1)/2 \approx N^2/2$) parameters as there are observations ($NT \approx N^2$). FGLS works because, asymptotically, OLS residuals give us a good estimate of Ω. With only about two observations per parameter, we cannot have any assurance that the estimated $\hat{\Omega}$ has any relationship to Ω. The Parks procedure will reliably estimate the standard errors of the model only when T is much larger than N.

Freedman and Peters (1984) have shown that FGLS must underestimate the true standard errors. Two of us (Beck and Katz 1993) have undertaken a Monte Carlo analysis of the properties of FGLS and OLS (with robust errors) for typical cross-national panel data situations. These show that the FGLS underestimate of standard errors may be severe, and the efficiency gain slight, for typical cross-national panel situations.[9]

In particular, "95% confidence intervals" computed using FGLS contain the true parameter value only about half the time when N = 18 and T = 22; this increases to only about three times in four when N = 15 and T = 28 and is still below 90% when N = 15 and T = 40. In the first case, FGLS estimates of standard errors are only about a third of their true (simulated) sampling variation; when N = 15 and T = 28, estimated FGLS standard errors are still only 60% of simulated variability; even when N = 15 and T = 40, the FGLS standard errors are still only about 80% of the simulated variability. Since cross-national panel studies typically have Ns and Ts in this range, these Monte Carlo results indicate that the Parks method (FGLS) may not be ideal for this type of data.

The OLS standard errors, on the other hand, are good indicators of parameter sampling variation even in the presence of extreme contemporaneous correlation of the errors. In the worst case, the OLS standard errors overestimate sampling variability by under 20%; for moderate amounts of contemporaneous correlation, the OLS standard errors approximate sampling variability well. In any event, the robust standard errors invariably do a good job at estimating the variability of the OLS estimates.

The Monte Carlo study also shows that FGLS is more efficient than OLS only when there is a high level of contemporaneous correlation of the errors. But even in the worst case for OLS, it is 80% as efficient as FGLS. For moderately correlated errors, OLS is just as efficient as FGLS; and OLS is more efficient than FGLS when the errors show little contemporaneous correlation.

In short, when the number of observations per cross section is less than the number of cross sections, FGLS cannot be used. Also, unless the number of observations per cross section greatly outweighs the number of cross sections, FGLS does a poor job of estimating the uncertainty in the model coefficients. The Monte Carlo results suggest that the method used here, OLS with robust standard errors, performs well. Thus we can trust our main positive finding that economic growth is enhanced by a fit between labor organization and political arrangements. The Monte Carlo study also indicates that the method used to obtain this finding is not simply a bad compromise in the presence of limited data but, rather, a method that should perform well for much cross-national panel data.

Notes

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1. We used an unsupported procedure, TSCREG, in SAS Version 5.18 (SAS Institute Inc. 1986). TCSREG produced, without warning, estimates of standard errors that are merely artifacts of rounding error. The SAS routine should not have computed any estimates, given our data set. Other programs that estimate similar models, such as SHAZAM (White, et al. 1990), correctly stop in the presence of insufficient data.

2. First differences instead of levels were used by AGL to deal with possible nonstationarity of the unemployment and inflation series.

3. All variables are as defined by AGL.

4. The reanalysis uses the sample period 1971–84 because of the use of a lagged dependent variable. This change has no consequence for either the statistical argument or the substantive findings.

5. The rank of **E′E** is the rank of **E**. If $T < N$, the rank of **E** is T; but **E′E** has N rows and columns, so it must be singular.

6. The Lagrange multiplier test takes into account the panel structure of the data. For economic growth, the test statistic is .44. Since, under the null hypothesis, the Lagrange multiplier statistic has a chi-squared distribution with one degree of freedom, we clearly cannot reject the hypothesis that the errors are not serially correlated. The estimated serial correlation is under .12, with a standard error above .07.

7. Heteroscedasticity is not a serious problem in the AGL data because their dependent variables were growth rates and changes inflation and unemployment.

8. We have not found in the literature any applications of the White procedure to panel studies, but the generalization seems clear and the conditions for White's procedure to provide consistent estimates hold quite easily. The White procedure has only an asymptotic justification, but our large sample size (224) allows us to use asymptotic results with some confidence. The robust errors reported here correct only for contemporaneous error correlation and heteroscedasticity, because our data show no serial correlation. The White method can be extended to handle serially correlated errors.

9. The Monte Carlo results reported here are based on the experiments reported in Beck and Katz (1993). Details on all the experiments, as well as GAUSS code, are available from Beck. The results reported here deal only with problems caused by contemporaneously correlated errors.

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A NOTE ON THE IDEA OF THE MORAL ECONOMY

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In their research note Robert Bates and Amy Curry (1992) have drawn our attention to the importance for political science of the challenges posed by the moral economic approach. Using their essay as a springboard and recasting their understanding of the idea of the moral economy, I show that the full significance of this controversy would be better illuminated by moving the debate on to a different terrain, one that captures the fundamental sources of the antagonism between the moral economic and competing approaches. I begin by setting out the central difficulty in Bates and Curry's reading of the moral economy argument and relate that to how the controversy has been framed in the recent political science literature. I then offer a sketch of the core claims of the moral economic approach, drawing out the radicalness of their challenge, and offer an estimate of their weaknesses and value. I conclude with some observations about political philosophy and the idea of the moral economy.

Few controversies are as long enduring as those which turn around the work of the moral economists, their counterparts in other disciplines, and their critics. Indeed, at the very beginning of Western reflection on politics, Aristotle framed the issues in a way that has yet to be surpassed: was the community an economic association or a community in the (noneconomic) things of the good life, above all, in those bound up with a shared perception of justice? Forerunners of these debates are also to be found in nineteenth-century disputes over the understanding of ancient Greek society and in Marx's polemics against classical political economy with its (alleged) tendency to read all human history as a market populated by profit-maximizers. In our century, the issue was rejoined with the publication of Karl Polanyi's *Great Transformation* in 1944. At first a debate among anthropologists ("formalists" against "substantivists"), these arguments have resonated so widely that there is now hardly an area of social research—from the history of ancient Greece and Rome to the study of peasant villages in Vietnam, from the analysis of the household to debates over rational choice theory—that has not been influenced by them. The wide theoretical radius that these debates have achieved is a function of the fact that while they often focus on the explanation of more or less explicit and readily recognizable forms of economic organization (e.g., markets, corporate villages), at their heart is something more general, namely, the standing of the economic approach to human behavior and of market society. Can theories of the self, of self-interested behavior, of maximization and its associated conceptions of rationality be used to interpret the actions and institutions of people in nonmarket societies or in nonmarket institutions within market society (e.g., the family) or even, for that matter, of market actors themselves? Intertwined with this is a normative debate over the status of markets—attempts to recover from the premodern world alternatives to our own institutions or to show the moral superiority of the market to other forms of economic coordination.

Bates and Curry (1992) have done us the service of focusing our attention once more on the importance of arguments concerning the moral economic and alternative approaches. In so doing, they have also managed to formalize (and thus make more precise) some of the notoriously fuzzy contentions of the moral economists. And they have elaborated a potentially rich critique of certain aspects of that literature. What I wish to suggest here, however, is that Bates and Curry have not gone to the core of the moral economists' argument. Thus their critical engagement with the latter, while telling within its own horizons, leaves the heart of the issues untouched. I make this case not in order to defend the moral economists but to show that Bates and Curry's agenda, which is a vital one for political science, would be better served by moving the debate on to a different terrain than the one they chose. I shall begin by setting out the central difficulty in Bates and Curry's reading of the moral economy argument and relate that to how the controversy has been cast in the recent political science literature. I shall then offer what I hope is a more exact identification of the principal claims of the moral economic approach and sketch its weaknesses and value. I shall conclude with some observations on the challenges posed by their arguments.

The basic difficulty in the Bates–Curry piece is fully on display in the tension contained in the following clutch of propositions, ventured in two consecutive paragraphs of their research note. The moral economists, they maintain, are committed to (1) an emphasis on the "high levels of risk that confront agrarian societies," a risk magnified by market price fluctuations that may threaten a loss of subsistence, and (2) the view that collective (peasant) villages are "social as opposed to economic institutions" and that accordingly, the "allocation of factors of production in collective villages is determined by social relationships rather than by markets" (1992, 458). Bates and

Curry then state that according to the moral economists, these communities "do not allocate resources so as to maximize total output" but, rather, in order to secure "key values." The most important of these is the "security of subsistence," but they also include justice. The authors develop a critique of this showing that the dominant allocative institution of peasant corporate communities (*the Rule*) does not, in fact, ensure subsistence or preserve other values better than does the market, as the moral economists assert. Consequently, the sources of peasant rebellion must be sought elsewhere than in the breakdown of peasant institutions such as the Rule.

Propositions 1 and 2 are inconsistent with each other. Only proposition 2 expresses (and then loosely) the centrepiece of the moral economic argument. Consider, first, proposition 1. Unfolded, this tells us that (a) many agrarian societies live at the margin of subsistence, (b) market-induced price fluctuations pose a high level of risk to subsistence, (c) village communities adopt nonmarket economic institutions in order to ensure against that risk. My concern is not whether claims a–c are, in point of fact, accurate. Rather, what needs to be said about them is that (empirically right or wrong) as a mode of explanation of the presence of nonmarket institutions in agrarian communities, they are entirely consistent with the economic approach to human behavior, including a choice-theoretic approach. What they do is set out an economic explanation of the dominance of nonmarket institutions in these societies by pointing to the costs of markets. Such an explanation would, broadly speaking, be of a piece with other "formalist" (to use Polanyi's pejorative term) efforts to account for nonmarket institutions, for instance, some theories of the firm, North's work on premarket societies and economic change, Posner on justice and the state in early societies, and Bates himself on Africa (Bates 1983; Coase 1952; North 1977, 1981; Posner 1981). Indeed, only if one gives the idea of the economic an extremely narrow construction (e.g., competitive profit seeking or maximizing "total output"), for which the moral economists are often and correctly criticized, is this not an economic explanation of nonmarket institutions. Having thus moved the moral economist's argument on to the theoretical terrain of political economy, Bates and Curry's critique becomes, in effect, an immanent one conducted within a shared conceptual horizon.

However, the explanatory structure of proposition 1 sits very uneasily with that proffered in proposition 2; and a critique that might well be successful against the former scarcely engages the latter at all. In proposition 2 we are told that according to the moral economists, the economy in these collective villages is "determined by social relationships rather than by markets" (Bates and Curry 1992, 458). The phrase "social relationships" is murky, but its import becomes plainer when it is made to stand in contrast to markets and "economic institutions." A social relationship might be kinship, friendship, or citizenship; and to say that the allocation of factors of production is determined by such relationships would be to make a claim very different than that set out in proposition 1. Seen from this theoretical perspective, other dimensions of the community (e.g., solidarity, hierarchy) would trump economic calculation in the sense that scarcity, optimal employment of assets, and so forth would not be the governing criteria. That governing function would belong to aspects of the social relations among the community's members.

Here is an illustration. It was common in classical Athens (and in the contemporary societies studied in Scott 1976 and Popkin 1979) for the wealthy to redistribute a portion of their money to the poor, often without direct taxation. In predemocratic Greek society, this took the form of noblesse oblige, personal and direct gifts from the well-to-do to the poor. In democratic Athens, such aristocratic practices, tainted by deference between the donor and the receiver, were no longer consistent with social relationships; and so their form was changed to gifts to the public, or liturgies. Let us imagine (only partially faithful to the historical record) that these transfers in fact served to shield the poorest members of that society from extreme deprivation. An explanation of this, along the lines of proposition 2, would say that distribution in this community was "embedded" in a nexus of social relationships that determined duties on the one side and expectations on the other and that the specific organizational forms of this redistribution are the result of shifts in the political institutions and their respective ethos—from the ethos of an aristocratic class to that of an egalitarian democracy. Something like that would be the analysis of a moral economist.

However, the analytical path marked out by proposition 1 is fundamentally different, and inaccurate for the moral economist. Its determinants are scarcity, choices among feasible alternatives, and the cost-weighting of those alternatives. Here is a sketch of the principles of a type 1 account of nonmarket distributive mechanisms. Traditional forms of economic organization, such as gift giving or liturgies, may be chosen because they are in the "economic self-interest" of a group. And the move to alternative mechanisms (e.g., markets) might be rejected because of the costs associated with the transition to that new regime: preservation of traditional structures may minimize conflict; and in so doing, it can help to reduce the costs of enforcing social order. Or markets could be rejected because of their costs once set up (e.g., property rights regimes). The nonmarket arrangement is chosen; and the criteria governing that choice are economic, broadly understood. (For a type 2 explanation of ancient economic behavior, see Finley 1973, 150–65. For type 1, see Gunderson 1982, 237–38.) In brief, then, explanation types 1 and 2 are inconsistent with each other and only type 2 accurately captures the moral economists' position. Bates and Curry's critique works, in large measure, because the theses at which they are taking aim are not part of the moral economic explanatory structure.

Bates and Curry's note does give a fair account of

James Scott's (1976) seminal study, *The Moral Economy of the Peasant*. But it is precisely because they have chosen that book to represent the moral economy literature that they find themselves in the conceptual knot just discussed. Indeed, the tension observed earlier between Bates and Curry's propositions 1 and 2 is first played out in Scott's own work. Here, in schematic form, is his version of explanation type 1. Scott's "basic idea" is that the peasant household's "primordial goal" is "reliable subsistence" and that this central economic dilemma causes the household head's risk aversion, and thus also his aversion to markets. This "safety-first" principle "lies behind a great many technical, social, and moral arrangements of a precapitalist agrarian order" (pp. 4–5). In short, the precapitalist community was organized to minimize risk and the safety-first principle makes good economic sense under conditions of subsistence agriculture. Scott suggests that the economic rationality of this principle may also be seen, mutatis mutandis, in the risk-minimizing behavior of corporations. Where subsistence security is not so overwhelming a problem, the peasant is likely to become more bourgeois. For similar reasons, where the state takes upon itself the role of securing an income floor, it becomes more natural for the peasant to engage in profit-maximizing behavior (pp. 9–10, 24–25, 29). The corporate village and its form of organization are thus an answer to an economic problem—scarcity—and to the risks involved in alternative institutions (e.g., markets). Relax the scarcity constraints either by stipulating greater wealth or a state that ensures peasants against risk, and peasants select different institutions and behaviors. This is an economic explanation of the nonmarket organization of agrarian communities. In Scott's words, "Peasant economics is better understood as a special case of what standard microeconomic theory would predict" (p. 14). (Taylor [n.d.] emphasizes this part of Scott's argument, just as Popkin [1979] focuses on its other, moral economic, dimension. In fact, what is perplexing in Scott's book is the presence of both approaches.)

That explanatory model rests uncomfortably with the second strand of Scott's analysis, one that mirrors the Bates–Curry explanation type 2—so uncomfortably, in fact, that it is evident in the author's own words: his analysis, he writes, "which begins with the peasant household budgets" and "deduces peasant needs and interests from them" "runs the risk of . . . 'methodological individualism'." The meeting of subsistence goals is fundamental; "but to stop there is to miss the critical social context of peasant action," that is, "culture," "a shared moral universe, a common notion of what is just" (1976, 165–67). Now it is this "shared moral universe" (a "moral vision," as Scott elsewhere calls it) that is said to explain why peasants rebel (p. 192)—whereas the previous observations might have suggested the threat to subsistence (said to be posed by markets) as the prime motivating force. It is never made clear how these claims are to be harmonized with explanations of type 1, such as Michael Lipton's assertion that many "odd village practices make sense as disguised forms of insurance" (quoted on p. 5). We are thus left with the impression that the "moral vision" is an overlay, a normative gloss that is rarely employed in Scott's analysis of agrarian communities. (One such exception would be his allowing for the "historical and cultural dimensions" of the idea of subsistence on pp. 16–17). I should add that his account may well be the better for that, but to read *The Moral Economy of the Peasant* as a guide to the moral economic approach or as a foil for criticizing it is to miss its core. This is not to say that Popkin, Bates, and Curry are wrong in seeing traces of the moral economic approach in Scott's work. His statement of indebtedness to Polanyi is one such trace, but more substantial evidence is to be found in Scott's emphasis on the commodification of land and labor (pp. 5, 7) and, above all, in the "culture" of the moral economic literature that pervades his book: an aversion to markets, the notion of the emergence of markets as a watershed transformation radically distinguishing modern society from its predecessors, and the sense that this watershed represented a loss. But this culture is far from directing his explanation of agrarian communities, which is, rather, guided by ideas of risk and insurance in the selection of economic institutions. It is for that reason that the debate between Scott and Popkin and between Bates, Curry, and Scott is (as I have suggested) an immanent one, conducted within the borders of the economic approach to human behavior and society.

If it is plain to see that Bates and Curry are not directly challenging a moral economic argument, it is more difficult to state precisely the core contentions of that latter school. Here I seek to map out only these key propositions and possible avenues of criticism of them, including lines that emerge from the Bates–Curry note. The central thesis of the moral economy school—and the wellspring of much of the rest of its claims—is that the economy is an "instituted process," one in which various institutional forms "integrate" economic activity (see Polanyi 1960, 329; Polanyi 1977, 35). This claim is then unfolded to say that there are, historically, three such forms of integration: redistribution, reciprocity, and exchange. Only the last involves markets, and these are by no means necessarily of the "pervasive" type (i.e., including markets for labor and land). Lastly, this third crucial gloss is deployed: the production and distribution of human livelihood have been integrated along two basic lines—"embedded" in social (read "noneconomic") institutions and disembedded—via the autonomous and pervasive modern market. I shall not attempt to clarify in detail the ideas of an embedded versus an autonomous market integration of the economy. (In fact, I suspect that they would resist such analysis.) For the present purposes, it will do to say that an embedded economy is one in which the securing of human livelihood is submerged in and determined by a nexus of noneconomic institutions: kinship, citizenship, hierarchy, and so forth. They are, in a loose sense, regulated economies—sometimes by a central power (redistributive societies) but in all cases by overdetermining and institutionalized norms, such as reciprocity. Under these "forms of integration," scarcity, economizing choices driven by scarcity, rational action, and so on—in brief, the elementary tenets of modern economic analysis—are essentially misleading (Pearson 1957, 320, 326; Polanyi 1977, 19–30). Indeed, the embedded or submerged nature of the economy in these societies means that the economy as a theoretically distinct sphere of phenomena does not exist; thus it cannot be conceptualized independently of the surrounding noneconomic institutions: it is "a mere function of social organization" (Polanyi 1957, 49, 54; Polanyi 1977, xlvii; Tribe 1978, 33, 36). For that reason, these authors claim, there was no discipline of economics before modernity. Accordingly, to import the language of scarcity-driven economizing choices back into these societies is to do conceptual violence to them, to disrupt the embeddedness of the securing of human livelihood that is their hallmark—in brief, to make them speak in an idiom not their own and incomprehensible to them for whom economic relations (as we name them) "always form part of direct social relationships of a general kind" (Evans-Pritchard 1940, 90; see also Dalton 1961, 1, 4–7).

Modernity, on the other hand, is characterized for the moral economists by the dominance and pervasiveness of the market, of "production for gain." Two features are of signal importance to them: its inclusion of labor and land as commodities (hence the pervasiveness of the market) and its self-regulating character, by which they mean that it functions, invisible-hand-like, without "outside interference." That last mentioned trait is its "most startling peculiarity" and what makes modernity out as an "utterly new type of economy" (Polanyi 1957, 41, 55; Polanyi 1977, 10). Much of Polanyi's argument from that point on is taken up with showing that the consequences of this self-regulating "juggernaut" were such that the visible hand of the state was brought back to save society from the market and that this institution, to the extent that it still integrates society, poses a fundamental threat. Briefly, the explanatory implication of this view of the disembedding of the economy is that the concept of the economy, as a clearly demarcated set of phenomena governed by its own laws, emerges only with market society. Thus it is there that we find, for the first time in human history, a purely economic science. In other words, only here are the "formal" concept of the economy and the science built upon it applicable, that is, scarcity-driven choice, maximizing behavior in a lawlike institution but one not regulated by human beings (Polanyi 1968, 59–77; Polanyi 1977, 6).

I reiterate that the thicket of ideas that I have just sketched is extensive and, in important ways, tangled and confused. Critics have correctly drawn attention to (among other flaws) the thinness of its definition of the economic and the restrictive understanding of the market (North 1977, 710). Some have challenged the notion of the "autonomous" economic sphere and the idea that maximizing behavior was not important before capitalism (Veyne 1974, 1376–77; Veyne 1976, 69–74). Other critics have pointed to the virtual absence of an explanation *why* this or that form of integration emerges (and, relatedly, why markets emerge) in the first place (see Godelier 1974, 1371–72; Hechter 1983, 168, 183). But the Bates–Curry critique, because it addresses arguments distant from the core of the moral economic approach, does not get us as far as it might into the fundamentals of the debate over that approach. What I want to do, therefore, is to outline a way of expressing the heart of the moral economic challenge and to offer that as a fertile terrain for further argument. The following paragraphs are thus by no means an exhaustive account of these debates but rather a promissory note—a suggestion as to the richness of the theoretical veins yet to be developed.

The moral economic challenge can be developed by unpacking its conceptual center, namely, the idea of an embedded economy—the nonmarket integration of economic activity through a variety of institutions of redistribution, reciprocity, and (limited) exchanges. Among the many possible avenues that this venture might take, the question of interpretation across radically different social formations seems particularly important: (1) as a challenge to the economic approach to human behavior, (2) as intimating the possibility of an economic regime normatively superior to market society, and (3) as an alternative language for framing moral and critical thinking about the economy. Here is a sketch of each of them in turn.

The Challenge of Explaining Nonmarket Forms of Integration. As we have seen, one response to this challenge has been to suggest that there is an economic explanation of nonmarket institutions, drawn from transaction-cost economics/theory of the firm (Bates 1983, 139; Hechter 1983, 173, 187; North 1977, 709, 713; North 1981, 42–43, 106, 181). Yet this attempt to tease an economic explanation out of a question designed to undermine just such an approach serves simply to force the issue to a more fundamental level. For, as North recognized, it was a cornerstone of the moral economist's argument that these embedded forms of economic integration are not explicable in the language of economizing behavior (1977, 707–8). Thus to allow that before capitalism economic activity had been mediated through networks of, for example, reciprocity rather than markets but then to account for this by saying that such an arrangement represents "the least cost trading solution where no system of enforcing the terms of exchange exists" (ibid., 713) is, from the perspective of the moral economist, theoretically to disembed the behavior of actors in those economies and to remake them into *homo oeconomicus*. To argue, for example, that market competition induces firms to run some or all of their internal operations in an authoritarian, nonmarket way is one sort of (plausible) claim; to suggest that gift-giving rituals among the lords of Homeric Greece or redistributive practices among Vietnamese peasants are available to the same structure of explanation is, at its root, distorting. In this sort of translation of their idiom into our own it may happen that, in Clifford Geertz's words, "things get lost" (1983, 10). It is that worry that leads us to the debate over the meaning and range of the economic approach and, ultimately, to the most crucial questions of method and interpretation.

The Possibility of a Normatively Superior, Embedded Society. As is evident from this analysis, much of the current debate is among scholars concerned with developing areas. But I have also indicated its influence across disciplines, for example, in anthropology, sociology, classical history, and economic history. In light of that considerable breadth, it is odd that the controversies surrounding the idea of the moral economy remain relatively untouched in political philosophy, though the increasing prominence of rational choice theory has brought political philosophers and others into aspects of these controversies (see Gauthier 1986; Hampton 1986; Mansbridge 1990). This is doubly odd because, in addition to the deep interpretative questions raised in the exchanges between the moral economists and their critics, it is also abundantly clear that since its inception in Aristotelian political philosophy, there has been a normative thrust to the idea of the moral or embedded economy. One construction of that normative dimension is, naturally enough, the issue of markets and the satisfaction of an array of human goods. The moral economists' claim that the emergence of market society was a watershed is much more than a historical statement. It is closely allied with the assertion that markets threaten life (in the case of the near-subsistence level communities studied by Bates, Popkin, and Scott) and (in that other, paradigmatic example, Aristotle and ancient Greek society) the possibility of the good life. And it is sometimes associated with a case for the "reembedding" of the economy. Interesting also is that despite significant parallels with Marxism, the moral economic project is as critical of the latter as it is of classical and neoclassical economics, seeing in its emphasis on economically defined class and modes or relations of production a mirror image of the market-derived dominance of *homo oeconomicus*. It is, in other words, a non-Marxist critique of market society and in its more intriguing formulations, it seeks to develop theories of reciprocity and altruism (for example) at the level of society as alternatives to the market (see, e.g., Kolm 1983, 1984; Titmuss 1971). Critics of the moral economic approach have also engaged in the normative aspects of these controversies, often by challenging the notion that the world before capitalism was more humane, less calculating, and less stratified than ours is (see Popkin 1979, x, 2, 6, 17, 28, 61). However, given that critics and proponents alike have placed more weight on the methodological and interpretative side of the scales, the normative facets, which plainly were a powerful initial spur, remain relatively underdeveloped. Thus with the exhaustion of Marxism, the moral economy debates may well provide fertile ground for reformulating the issue of the market as against other forms of economic organization.

The Possibility of a Better Normative Theory. Another natural reading of the idea of the embedded economy is that it is subordinated—not only in an institutional sense but morally, also—to the (noneconomic) ends of the community. This twofold meaning of the location (structural and normative) of the economy within the framework of the community suggests a teleological—a broadly Aristotelian—way of thinking about the economy. In place of the avowedly nonethical character of much contemporary economic thought, the idea of embeddedness virtually demands such an orientation as an essential part of understanding the economy. And in place of the received horizon of normative economic analysis, framed almost wholly within the language of rights (of the range of exclusionary rights over one's self and over the things of the external world), the moral economic approach attempts to restore the question of the telos, of the good to be served by the economy—in a phrase, of its moral embeddedness. (I explore some of these issues in Booth 1993.) Here its methodological and normative dimensions converge in a challenge both to economics as a science and to liberal theories of the economy, a challenge that is, in the same moment, a call for an economic science that takes as its center the location of the economy in the architecture of society—its institutions and values—and an appeal for a new normative theory, one guided by the question of what end or good the economy ought to serve.

Each of these three paths virtually demands further development and, needless to say, critical response. And it is Bates and Curry's achievement to have pointed us to these important and still unsettled controversies, debates that go to the heart of a range of cross-cutting issues in the social sciences and political philosophy. We have also seen, however, that the questions raised by the idea of the moral economy are not, at the most important level, joined in the exchange over whether corporate villages or markets provide the best risk insurance to subsistence-level peasants. What remains to be done, then, is to cast the moral economic approach in a manner that draws the foundations of the debate into plain view. For all those reasons, the Bates–Curry note is a welcome invitation to reflection.

Note

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ELECTORAL COMPETITION IN THE AMERICAN STATES

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Electoral competition is a concept that has played a central role in much of the state politics literature. One commonly used indicator of competition in the states is the Ranney index. We offer an alternative indicator of competition, one based on district-level outcomes of state legislative elections. After evaluating both indicators in terms of validity and reliability, the analysis suggests that the district-level indicator is both empirically and intuitively superior as a measure of electoral competition. The implications of this finding are discussed.

Electoral competition in the American states has generated more debate and research than perhaps any other concept in the field of American state politics. The interest in competition began with the regional studies of Key (1949) and Lockard (1959) and has expanded to dozens of articles attempting to link competition to policy outcomes, voter turnout, and other political phenomena. Much of the literature focuses on the influence of competition on policy outcomes. The guiding hypothesis for many studies has been that competitive political systems will display a tendency to produce more liberal policies than will noncompetitive political systems, a hypothesis derived from Key's 1949 *Southern Politics in State and Nation* (but see Uslaner 1978).

Though the theoretical underpinnings of this hypothesis are not always clearly articulated, we see the connection between political competition and liberal public policies as resting upon two interconnected assumptions. First, elected officials in competitive areas will be highly responsive to constituency needs, due to the risk of electoral defeat. Second, due to higher overall levels of voter participation in competitive environments, lower socioeconomic class interests will constitute a greater share of the electorate in competitive states than in non-competitive states.[1] Therefore, in striving to represent the interests of their constituents, elected officials in competitive states will provide benefits to lower socioeconomic interests to a greater degree—and will display a greater propensity to support liberal policies (policies for the have-nots, according to Key 1949)—than will elected officials in noncompetitive states, all else held constant.

While the empirical evidence is somewhat mixed, most studies indicate that competitive political systems do produce more liberal policy outcomes, though the effects are sometimes small when compared to socioeconomic variables (Dawson and Robinson 1963; Dye 1966, 1968, 1984; Holbrook and Percy 1992; Lewis-Beck 1977; Lowery 1987; Cnudde and McCrone 1969; Marquette and Hinckley 1981; Meier 1991; Plotnick and Winters 1985; Tompkins 1975; Walker 1969).

Another, less developed but related branch of research suggests that competitive environments generate higher rates of voter turnout. The logic underlying this relationship can be based either on individual-level incentives or on the incentives and behavior of candidates and political parties. First, close elections are likely to generate more interest and information, thereby reducing certain costs associated with voting. Close elections also provide a strong incentive for the candidates and parties to organize and mobilize the electorate (Powell 1986). Most empirical studies have supported the relationship between competition and turnout (Dye 1966; Milbrath 1971; Patterson and Caldeira 1983; Wolfinger and Rosenstone 1980), although there is some evidence to the contrary (Gray 1976).

The literature cited is only a sampling of the vast scholarship that has been generated on competition in the states. A number of indicators of competition have been developed and used over the past three decades (Broh and Levine 1978; Dawson and Robinson 1963; Golembiewski 1958; Ranney 1976; Zody and Luttbeg 1968). Perhaps the most commonly used indicator is the folded Ranney index (Ranney 1976). While the Ranney index has been subjected to considerable analysis (Barrilleaux 1986; King 1988; Patterson and Caldeira 1984; Tucker 1982), it—or variations on it—stands as the most widely used indicator of political competition in the states. We offer an alternative indicator of competition, one based on district-level outcomes in state legislative elections.

We shall discuss and compare the Ranney index and the alternative measures, then assess both measures in terms of face and empirical validity and briefly consider indicator reliability. Finally, we discuss the implications of the findings.

Ranney Index

The Ranney index is used as an indicator of the degree of two-party competition in the states. This index was developed in Ranney's (1976) "Parties in State Politics," originally published in 1965, and continues to be updated by Bibby and his colleagues (1983/1990). The original index is actually a measure of the strength of the Democratic party in state government. The Ranney index takes into account the proportion of seats won in the state House and Senate elections, the Democratic percentage in the gubernatorial election, and the percentage of the time the governorship and state legislature were controlled by the Democratic party. These factors are averaged together over a specified period of time, yielding a measure that ranges from 0 (complete Republican domination) to 1 (complete Democratic domination). The midpoint, of course, indicates evenly divided control, or (in the words of those who use the index) *perfect competition*. The utility of this scale as a measure of party competition is realized by folding the scale so the two noncompetitive extremes are brought together at the low end and the midpoint becomes the high point.[2] The resulting index ranges from .5 (no competition) to 1 (perfect competition).

The folded Ranney index has itself been the subject of a significant amount of analysis. One branch of research has focused on the sources of competition. Patterson and Caldeira (1984) found that competition (measured with the Ranney index) is greatest in large, diverse states with strong party organizations. Barrilleaux (1986) is critical of Patterson and Caldeira's cross-sectional approach and examines change in the Ranney index during the 1970s. Although there are some similarities, many of Barrilleaux's findings sharply contradict those of Patterson and Caldeira.

Another branch of research addresses potential problems with the Ranney index. Tucker (1982) suggests that one problem with the Ranney index is that it is averaged over periods of time that are too long to generate a stable indicator. King (1988) identifies the gubernatorial election component as a distinct dimension that does not fit well with the state legislative components. Both Tucker and King recognize some problems with the folded index but are still supportive of its use and suggest only minor modifications. Stonecash (1987) is much less generous, finding fault not only with the Ranney index but with any aggregate cross-state index of party competition generally.

District-level Competition

The measure of electoral competition offered here is based on district-level state legislative election results from 1982 to 1986.[3] Though rarely used, district-level competition has been examined before. Tidmarch, Lonergan, and Sciortino (1986) produced several measures of district-level competition in the states from 1970 to 1978. Unfortunately, their work was virtually ignored by others in terms of providing data for an alternative measure of competition in the states. Garand (1991) examined marginality in state legislative elections in 16 states from 1968 to 1986. Weber, Tucker, and Brace (1991) also examined the phenomenon of vanishing marginals in the lower houses of 20 state legislatures. To date, however, there has been no effort to produce a single, district-based measure of electoral competition in the states as an alternative to the Ranney index.

Like the Ranney index, the district-level measure is based on several components. First, we use the percentage of the popular vote won by the winning candidate. Second, we use the winning candidate's margin of victory. These first two components are not necessarily redundant. It is not uncommon in state legislative races to have more than two candidates in a race. Because of this, the winning candidate may have a relatively low vote percentage and still have a wide margin of victory over the second-place candidate. Third, we consider whether or not the seat is "safe." Though there is some disagreement on this matter, we follow the example of Tidmarch, Lonergan, and Sciortino and adopt a winning percentage of 55% or more as a safe seat. Finally, we consider whether the race was contested or not.

Any one of these components could serve as an indicator of district-level competition. We feel, however, that the best indicator is one that takes into account as much information as possible. Therefore, each of the several components are averaged across districts and combined into a single index value for each state from 1982 to 1986.[4] Complete absence of competition is indicated by a score of 0 on the district-level measure. This would be a case where all candidates are unopposed. As the scale increases from 0, it indicates greater competition. A score of 100, however, is theoretically impossible as long as someone wins the contests.

Table 1 presents the values for both the district-level measure and the Ranney index.[5] The states are ordered according to their value on the district-level measure. For both indicators, higher values represent more competition. Generally speaking, there is some overlap between the two indices, suggesting both that southern states tend to be the least competitive and that midwestern states tend to be the most competitive. There is significant disagreement, also. For instance, Delaware—which is tied on the Ranney index with North Dakota as the most competitive state—ranks only twenty-sixth in terms of district-level competition.

The relationship between the two indices is further depicted in the scatterplot in Figure 1. Note that there is considerable variation in district-level competition among those states ranked as the most competitive on the Ranney index. Also, many of the states with the highest district-level competition fall near the middle of the Ranney index. The correlation between the two indices (.68) is moderately strong but not particularly impressive if they are thought to be measuring the same concept.

The differences between these two measures of competition are of no small consequence. Both measures allege to be indicators of electoral competition in the states. We shall argue that in fact, the district-level indicator measures competition much more directly than the Ranney index. Then we shall compare the two measures in terms of both face and empirical validity.

VALIDITY

Whenever attempting to measure a concept indirectly through indicators, the crucial question is whether

TABLE 1

Measures of Political Competition in the States

STATE	DISTRICT-LEVEL COMPETITION	RANK	FOLDED RANNEY	RANK
North Dakota	56.58	1	1.000	1
Oregon	54.25	2	.900	14
Nebraska	54.06	3	.980	7
Washington	53.94	4	.830	26
Alaska	53.46	5	.970	9
Connecticut	52.81	6	.920	13
Minnesota	52.44	7	.860	18
New Jersey	51.81	8	.990	3
Ohio	49.61	9	.960	12
Nevada	49.60	10	.990	3
Michigan	49.58	11	.980	7
Vermont	49.16	12	.970	9
Wisconsin	49.13	13	.820	29
Iowa	48.55	14	.860	18
New York	47.68	15	.990	3
California	47.29	16	.840	23
Maine	45.90	17	.800	30
Utah	45.29	18	.750	39
West Virginia	44.97	19	.730	43
Indiana	44.59	20	.860	18
Montana	43.34	21	.990	3
Illinois	41.61	22	.860	18
Virginia	40.71	23	.760	34
Pennsylvania	40.19	24	.970	9
Colorado	40.18	25	.830	26
Delaware	39.66	26	1.000	1
Rhode Island	39.49	27	.780	33
South Dakota	39.19	28	.760	34
New Mexico	37.10	29	.830	26
Kansas	35.81	30	.890	15
Idaho	35.60	31	.800	30
Arizona	33.90	32	.890	15
North Carolina	33.42	33	.800	30
Hawaii	33.40	34	.730	43
Florida	31.13	35	.760	34
Maryland	31.00	36	.740	41
Wyoming	30.46	37	.840	23
Massachusetts	30.39	38	.760	34
New Hampshire	29.01	39	.840	23
South Carolina	28.32	40	.710	46
Kentucky	27.81	41	.730	43
Alabama	27.27	42	.660	49
Missouri	27.12	43	.880	17
Tennessee	26.72	44	.860	18
Oklahoma	25.49	45	.760	34
Texas	21.96	46	.750	39
Mississippi	16.48	47	.640	50
Georgia	16.19	48	.740	41
Arkansas	9.26	49	.700	47
Louisiana	—	—	.690	48

Note: States are listed in order of competitiveness on the district-level indicator (most competitive to least competitive). The district-level indicator is based on district-level state-legislative election outcomes from 1982–1986, with high values indicating high levels of competition. The Ranney index is based on party control of the state legislature and the governorship from 1981–1988 (Bibby et al., 1990), with high values indicating high levels of competition.

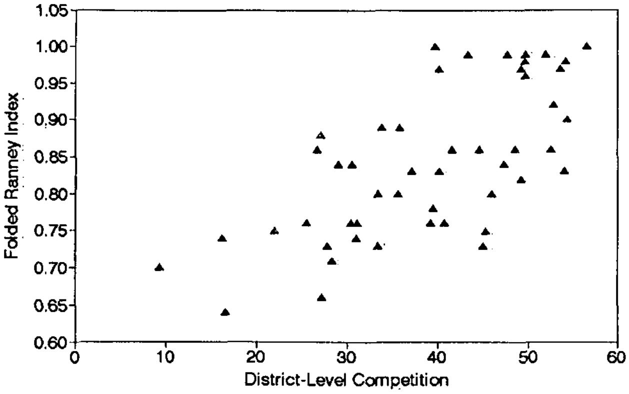
the indicators are valid representations of the concept. The validity of the Ranney index and the district-level indicator is assessed. First, we examine the indicators in terms of face validity. Next, the indicators are compared in terms of exterior criterion validity.

Face Validity

The issue addressed by face validity is whether, on the face of it, an indicator appears to be a valid representation of the concept. Of the two indicators

FIGURE 1

Plot of Competition Indicators.



used here, the strongest case for face validity can be made for the district-level indicator. Our reasoning is quite simple. The Ranney index is based largely on aggregate party strength in government, not on actual election results. The only part of the Ranney index that takes into account any individual election results is the gubernatorial election component. Recall that King (1988) found this component to be empirically distinct from the state legislative component. This is not surprising since one component is based on election results and the other is not. Even though the Ranney index does include the gubernatorial election component, it may not be prudent to accept this as an indicator of the degree of competition in the political environment. Given that only one gubernatorial election is held every four years in most states, it is possible that the results of this election may not represent the true degree of competition in the state. This leaves the Ranney index otherwise void of actual election results. In short, the Ranney index is really a measure of party control of state government, not the competitiveness of state elections.

The district-level indicator does not have the same problem. In fact, the district-level indicator is based entirely on actual outcomes in state legislative districts. Given how the indicator is computed, it is difficult to imagine a more direct measure of electoral competition in the states.

The consequences of these differences can be quite severe. Consider the hypothetical states presented in Table 2. In state A all legislative districts are very competitive, with all winners barely escaping defeat. Although all districts are competitive, the Democrats happen to win all four seats. This produces a Ranney index value of .5, which indicates absence of competition.[6] This would appear to be a ludicrous finding, given the level of competition existing in all four districts. Certainly, none of those elected in state A would feel that they were in a noncompetitive situation. For the same state, the district-level indicator produces a value of 86.13, suggesting a very competitive environment. (Using the district-level indicator, the most competitive state in Table 1, North Dakota, has a value of 56.58.)

A similar pattern is found in state B, where none of the districts are competitive but control of government is evenly divided. The district-level indicator produces a value of 8.76, making state B about as competitive as Arkansas (see Table 1). However, because control of the legislature is evenly divided, the Ranney index produces a value of 1.0, indicating perfect competition. Is it likely, however, that any of the elected officials in state B feel the pressures of electoral competition?

The hypothetical cases in Table 2 point to the major shortcoming of the Ranney index: it is frequently used as a measure of electoral competition even though, for the most part, it is not based on election results. A cynic would argue that these hypothetical

TABLE 2

Hypothetical Outcomes for Different Measures of Competition

		STATE A		
	DISTRICT	DEMOCRATIC%	REPUBLICAN%	WINNER
House	1	50.1	49.0	Democrat
	2	50.2	49.8	Democrat
Senate	1	50.1	49.9	Democrat
	2	50.3	49.7	Democrat
		Ranney = .50	District Indicator = 86.13	
		STATE B		
	DISTRICT	DEMOCRATIC%	REPUBLICAN%	WINNER
House	1	83.5	16.5	Democrat
	2	75.5	24.5	Democrat
Senate	1	0.0	100.0	Republican
	2	5.3	94.7	Republican
		Ranney = 1.0	District Indicator = 8.76	

examples do not represent what is likely to occur in the real world. Yet an inspection of Table 1 and Figure 1 reveals that there are substantial discrepancies between the Ranney index and actual district-level outcomes. In fact, the states that have Ranney index values of .97 or higher (very competitive) averaged 9.1% uncontested seats from 1982 to 1986.[7] In two extreme cases—Delaware (Ranney = 1.0) and Montana (Ranney = .97)—28% and 26% of the races are uncontested, respectively. Again, these discontinuities are to be expected, given that the Ranney index is not based on election results.

Another difference between the two measures is that the Ranney index is specifically oriented to two-party competition, whereas the district-level measure is based on electoral competition, regardless of the parties of the winners and losers. This partisan component does not give the Ranney index any advantage over the district-level measure. The important point is that to be useful, both indicators should represent the degree to which elected officials feel pressure from the electorate. As Fenno points out, "As long as representatives want to retain office, the knowledge that they will later be held accountable at the polls will tend to make their representative behavior more responsive to the desires of their constituents" (1978, 233). Which indicator is likely to send a signal to representatives that they may be held accountable at the polls? Tidmarch, Lonergan, and Sciortino argue that aggregate party balance in the legislature, such as that measured by the Ranney index, is of little relevance to the individual legislator as a sign of competitiveness. According to them, state legislators are more likely to "devote effort to comprehending the strength or weakness of their own candidacies and those of their adversaries and colleagues" (1986, 356). The examples we have provided show that the Ranney index does not measure these attributes, while the district-level indicator does. This difference gives the district-level measure stronger face validity.

Exterior Criterion Validity

While establishing face validity is very important, it is also a terribly imprecise enterprise. A more precise, empirically based test for validity is *criterion-related validity*. The process of establishing criterion-related validity involves identifying external concepts that are expected to be related to the concept being measured—in this case, electoral competition. One then checks for correspondence between indicators of the external concepts and the indicators being analyzed. The degree of correlation between the indicators is taken as a measure of the validity of the items being analyzed (Carmines and Zeller 1979).

The criterion variables used in this analysis fall into two categories: indicators of public policy output and indicators of political participation. The expectation is that indicators of electoral competition are positively related to liberal public policies, or policies for the "have-nots" (Key 1949), as well as to political participation. Several measures of policy liberalism are used to test the relationship between the two indicators of competition and public policy outputs.

AFDC adequacy is an indicator of Aid for Families with Dependent Children (AFDC) effort that takes into account how close AFDC money brings recipients to per capita state income (Albritton 1990).

The Green index is an index of the stringency of a state's environmental protection laws. Based on 67 different environment policies, high values indicate greater stringency (Hall and Kerr 1991).

Tax progressivity is a ranking of the states according to the progressivity of their tax policies (Hansen 1990). Low values indicate more progressive taxes.

Consumer protection is the number (out of 16) of consumer protection measures enacted by 1981 (Meyer 1982).

Medicaid generosity is an indicator of medicaid generosity based on eligibility requirements, service, and reimbursement policies (Colby and Baker 1988). High values indicate greater generosity.

Disability rights is a measure of the stringency of state disability rights laws (Holbrook and Percy 1992). High values indicate more stringent laws.

Another policy-related consequence of political competition is what Dye (1984) has termed the "policy relevance" of parties. Dye defines a policy-relevant party system as one in which changes in party control of government are followed by corresponding changes in public policy (Democratic control, liberal policies; Republican control, conservative policies). In other words, in policy-relevant states, party control of government has policy consequences. One of Dye's findings is that party competition seems to facilitate the relationship between party control of government and public policy: competitive states tend to have policy-relevant parties to a much greater degree than do noncompetitive states. Therefore, when control of government changes in competitive states there will generally be a corresponding change in policy. An examination of Garand's (1985) analysis of government spending patterns supports Dye's hypothesis. *Policy relevance* is a dummy variable, reflecting Dye's typology, scored 1 for all states with policy-relevant party systems, and 0 for all others (Dye 1984).

In addition to these policy measures we also assembled two indicators of political participation to use as criterion variables. *Voter turnout* is average statewide turnout in the 1984 and 1988 presidential elections (Bibby et al. 1990). *Voter registration* is the average of the percent of the voting-age public registered to vote in 1984 and 1988, according to *Statistical Abstract of the United States* for 1989.

Regression analysis is used to assess the relative criterion-related validity of the two indicators of competition. Each of the criterion variables is regressed on one of two different models. The policy variables were regressed on state per capita income, Democratic control of government (unfolded Ranney index), percentage of the state population living in

TABLE 3

Exterior Criterion Validity of the Two Measures of Political Competition

DEPENDENT VARIABLE	RANNEY INDEX		DISTRICT-LEVEL COMPETITION	
	T-SCORE	R^2	T-SCORE	R^2
AFDC Adequacy[a]	.86	.456	3.99**	.551
Green Index	.46	.502	2.44**	.562
Tax Progressivity	−.37	.274	−.72	.283
Consumer Protection	.19	.379	2.45**	.465
Medicaid Generosity	.48	.413	2.65**	.498
Disability Protections	1.92*	.221	3.03**	.332
Policy Relevance	1.30	.281	2.59**	.349
Voter Turnout[b]	.62	.552	2.42**	.615
Voter Registration	.39	.470	1.24	.487

Note: Table entries reveal the relative statistical significance and explanatory power of the two indicators of competition in regression analyses of policy outcomes and political participation.

[a] For the policy equations, the independent variables are: per capita personal income, percent of the population living in urban areas, Democratic control of government, opinion liberalism, a southern dummy variable, and political competition.

[b] For the participation equations, the independent variables are: percent of the population living in urban areas, percent of the population with a college degree, per capita personal income, the closing date for registration, a southern dummy variable, and political competition.

**sig. < .05.

*sig. < .10.

urban areas, opinion liberalism, a southern dummy variable, and political competition. The participation variables are regressed on the percentage of the population living in urban areas, percentage of the population with a college degree, per capita income, how many days before election day registration ends, a southern dummy variable, and political competition.[8]

Table 3 presents the results of the criterion-related validity analysis. Table 3, column 1, lists the dependent variables used in the analysis. Columns 2–3 present the t-score of the coefficient for the Ranney index and the R-squared for the equation using the Ranney index as the measure of competition. Columns 4–5 present the t-score of the coefficient for the district-level indicator and the R-squared for the equation using the district-level measure of competition. The significance of the t-scores for the competition variables and the R-squared statistic for the models can be used to assess the validity of the measures. For each criterion variable equation, the competition variable that produces the largest t-score and R-squared is considered to demonstrate the greatest criterion validity.

The findings in Table 3 are striking and suggest that the district-level indicator is superior to the Ranney index in terms of criterion validity. The t-scores for the district-level measure are significant and in the anticipated direction in seven of the nine regression equations. By comparison, the Ranney index demonstrates a marginally significant impact in only one of the nine equations. The equations using the district-level indicator also produced higher R-squared values, even in the two cases where the t-score for the district-level indicator was not significant.[9]

Clearly, the district-level indicator of competition not only has stronger face validity but also has stronger criterion-related validity than does the Ranney index. Given the results in Table 3, it is interesting to muse about the possibility that some of the weak relationships found in previous work on the consequences of competition may have resulted from inadequate measures of competition. Certainly, to the degree that previous work has relied on the Ranney index or a Ranney-like index, this is a very real possibility.

RELIABILITY

Having examined the validity of the two indicators of electoral competition, we now turn to the final stage of the analysis: judging the reliability of the indicators. The issue of reliability concerns whether the indicator produces consistent results over repeated measurements. If, over repeated measurements, similar results are given, the indicator is said to be a stable, or reliable measure. The particular reliability test we selected is the Spearman–Brown prophecy formula, which is based on the split-halves reliability test (Carmines and Zeller 1979, 411). In order to conduct the reliability test, it is necessary to measure both indicators at two different points in time. The measurements for the Ranney index were taken from 1974 to 1980 and 1981 to 1988 (Bibby et al. 1983, 1990). The measurements for the district-level indicator are taken from 1972 to 1976 and 1982 to 1986.[10]

The reliability estimates for the Ranney index (.77) and the district-level indicator (.94) suggest that both indicators are fairly stable but that the district-level indicator exhibits considerably more stability than the Ranney index. What this means is that competitive states in one time period will be more likely to show up as competitive in another time period on the district-level measures than on the Ranney index. Although electoral competition is subject to change, we expect such changes to be slow in nature. Therefore, we expect to see little change in the indicators.

CONCLUSION

Electoral competition plays a central role in American state politics. Besides the importance of competition to studies of policy, participation, and other political phenomena, it is important on purely normative grounds. Simply put, in the absence of competition, accountability suffers. Given its centrality in state politics, it is vitally important that quantitative studies be based on valid, accurate indicators of competition. The findings of such studies are, in large part, only as good as the indicators they use.

We undertook the task of comparing two measures of competition in the states—the Ranney index and a district-level indicator—in terms of both validity and reliability. On every front, the district-level indicator proved to be both empirically and intuitively superior to the Ranney index. The primary problem with the Ranney index is that it is based largely on aggregate party balance in state government, not on election results. Aggregate party balance is an important concept and clearly has implications for public policy, but it is not the same as competition, at least not the type of competition that is relevant to the individual legislator.

The consequences of relying on the Ranney index or on other indicators derived in the same manner may be quite severe. The evidence in Table 3 shows that the interpretation of the impact of competition on public policy is quite different when using the Ranney index than when using the district-level indicator. In the case of the Ranney index, we could conclude that competition was of little relevance to either public policy or voter turnout. With the district-level indicator we concluded just the opposite, that electoral competition was significantly related to both policy outputs and voter turnout. This raises the possibility that the mixed results of previous studies of the impact of competition on policy may have resulted, in part, from erroneous measurement of competition.

Clearly, the results of empirical research are greatly affected by how we attempt to operationalize complex concepts. We offer the district-level indicator as an alternative measure of electoral competition that we believe has strong face and empirical validity.

Notes

1. Hill and Leighly (1992) have developed a measure of class bias in state electorates that gauges the difference in turnout between upper- and lower-income groups in the states (high values indicate upper-class bias). This measure of class bias is negatively related ($r = -.45$) with the indicator of electoral competition developed herein, supporting the assumption that lower-class interests are better represented in the electorate in competitive states than in noncompetitive states.

2. The formula for the folded scale is $1 - \text{ABS}(\text{Ranney} - .5)$.

3. The data used to construct this index are provided by the Inter-University Consortium for Political and Social Research (ICPSR). Specifically, the data are taken from *State Legislative Election Returns in the United States, 1968–1986* and were collected by the ICPSR.

4. The formula for statewide values of the district-level measure is

$$100 - ((\text{average \%vote for winners} + \text{average margin of victory} + \text{\%uncontested seats} + \text{\%safe seats})/4).$$

The components of the index are taken from the legislative districts and averaged or percentaged for the entire state. Cronbach's alpha for the index is .89. Returns for Louisiana general elections were not available for the time period used in this analysis.

Due to the difficulty in calculating these components for multimember free-for-all (MMFFA) districts, the district-level measure of competition excludes these districts. One potential problem with this exclusion is that the MMFFA districts could experience significantly different levels of competition than other districts in the same state. Were this the case, the measure presented here would not be a fair representation of the competitive environment in states with a significant number of MMFFA districts. To test this possibility, we created a measure of competition for MMFFA districts based on the number of candidates per position. We then correlated this measure of competition with the level of competition in non-MMFFA districts (using the district-level measure already described) for all states that use MMFFA districts (Arizona, Indiana, Maryland, New Hampshire, New Jersey, North Carolina, North Dakota, South Dakota, Vermont, West Virginia, and Wyoming). The correlation between these two measures is .87, indicating that competition in non-MMFFA districts reflects the level of competition in MMFFA districts in states that use both types of districts.

5. The Ranney index values used here are from 1981 to 1988 (Bibby et al. 1990).

6. Note that although the Ranney index usually includes data from gubernatorial elections, the data in Table 2 only represent outcomes for state legislative races. Gubernatorial election returns are not included in Table 2 primarily because the district-level measure does not take them into account. In addition, the exclusion of such returns simplifies the table and helps to highlight the differences between the two indicators.

7. These figures are based on data used to compute the district-level indicator.

8. Per capita income (1987) and figures for voter registration and voter turnout are all taken from the *Statistical Abstract of the United States*. The closing date for registration is taken from *The Book of the States, 1988–1989*. The urbanism and education measures are taken from the 1980 U.S. Census; Opinion liberalism is taken from Wright, Erikson, and McIver 1985, 478–79; and the Democratic-control-of-government measure is taken from Bibby et al. 1990.

9. Critics might suggest that the inclusion of the unfolded Ranney index in the model as an indicator of Democratic strength in state government stacks the deck against the folded Ranney index by introducing high collinearity. Indeed, the unfolded index is more strongly related to the folded index than it is to district-level measure of competition. However, removing the unfolded index from the model does not improve the results for the folded Ranney index. In fact, in some cases, the results are even weaker when the unfolded index is removed form the model.

10. Vermont is excluded from the earlier period due to missing data.

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THE IMPACT OF THE GULF WAR ON THE INGREDIENTS OF PRESIDENTIAL EVALUATIONS: MULTIDIMENSIONAL EFFECTS OF POLITICAL INVOLVEMENT

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When the United States began its overt military conflict with Iraq in January 1991, the news media focused unceasingly on the Gulf crisis. Using national survey data, we show that this emphasis altered the ingredients of Americans' assessments of George Bush's performance. After the war, assessments were based more on beliefs about Bush's effectiveness in managing the conflict and less on confidence in his handling of other foreign relations matters or the domestic economy. Consequently, Bush's overall performance ratings increased dramatically following the war. We also show that the media's impact on political judgments was regulated by citizens' levels of political knowledge, exposure to political news, and interest in the war. Greater impact was associated with higher levels of knowledge and lower levels of exposure and interest. These findings challenge traditional views of these dimensions of political involvement and support a view derived from contemporary psychological theories of information processing.

The 1991 Gulf War was certainly a momentous event in recent history, and its impact on the American public is likely to have been multifaceted. For example, events in the Gulf may well have altered Americans' confidence in the U.S. military and their support for government spending on defense. Or the war may have changed public support for Israel, the Arab nations, or particular U.S. foreign policies in dealing with the Middle East. We shall focus on yet another potential effect of the war, namely, its effect on the process by which Americans evaluate their president's job performance.

Public opinion about presidential performance exerts powerful forces on policy formation and implementation in Washington. Opinion polls document a president's popularity across the country, and perceptions of his popularity in turn partly regulate his ability to control political events. Popular presidents have their way with Congress and the federal bureaucracy, while unpopular ones struggle much less successfully to shape legislative decision making (Neustadt 1960; Rivers and Rose 1981). Thus, the public's judgments of presidential performance are quite consequential.

A look at public opinion poll results suggests that the Gulf War may have had profound impact on Americans' views of George Bush's performance. As Figure 1 illustrates, Bush's approval ratings were moderate just before the war began, in October 1990, at about 55%. When, after a gradual massing of allied troops in the Middle East, the United States initiated air attacks on Iraq in mid-January 1991, approval ratings began to increase. And in March, by which time the military efforts were completed, approval ratings soared to nearly 90%. Although the following months saw a gradual decline in this positive sentiment, they were still near 70% even three months later, quite a high level.

What accounts for the dramatic increase in presidential approval in early 1991? Perhaps it was simply the military alliance's success at ending the Iraqi occupation of Kuwait. But if this was all that drove the approval increase, it is puzzling why the rise began in January, just after the war began but before it was clear that U.S. efforts would be successful. A second possible reason for the increase is a shift in elite discourse, whereby administration critics are quelled during international crises (see, e.g., Brody 1991). A third possibility is an increase in domestic cohesion among Americans in response to external threat (Sigelman and Conover 1981). We focus here on a fourth possible explanation, whose starting point is the theory of *priming*.

Priming and the Gulf Crisis

The notion of priming begins by acknowledging evidence from psychological research indicating that when people make judgments or decisions, they rarely take into consideration the entire array of available relevant evidence. Rather, because of the cognitive burdens imposed by a complete and comprehensive information search and integration process, people tend to "satisfice," rather than "optimize" (see Simon 1957; Simon and Stedry 1968). That is, they often derive their decisions from limited subsets of the available information pool so as to make satisfactory judgments without expending a great deal of effort.

According to the theory of priming, satisficing operates for political judgments just as it does for other sorts of judgments. When Americans are asked to evaluate the job performance of their president, an optimal assessment might entail gauging the president's performance in a wide array of policy domains and integrating those domain-specific judgments into an overall summary. More likely, according to the theory of priming, especially during a relatively speedy

FIGURE 1

Proportion of the American Public Approving of President Bush's Job Performance

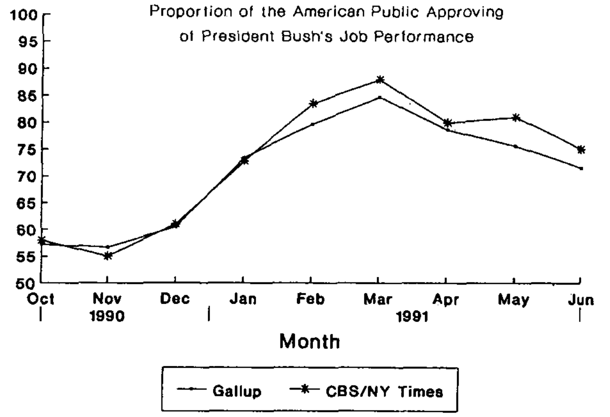
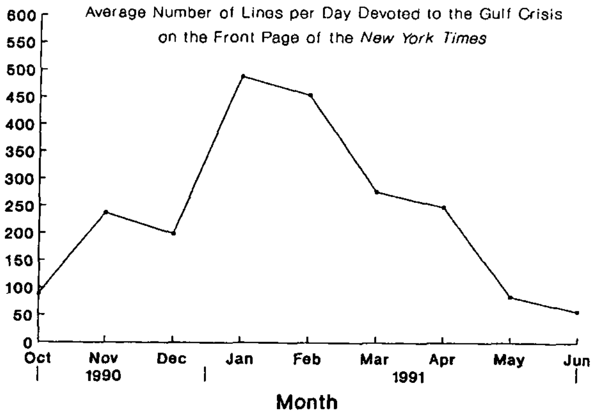


FIGURE 2

Average Number of Lines Per Day Devoted to the Gulf Crisis on the Front Page of the *New York Times*



survey interview would be a satisficing approach, assessing presidential performance in only a small sample of policy domains—a sample of convenience.

Which particular pieces of information get used are those that come to mind quickly and automatically for an individual citizen—those that are most accessible (Higgins and King 1981; Wyer and Hartwick 1980). And the accessibilities of various policy domains are in turn presumed to be determined importantly by news media coverage. Issues that have been the subject of extensive attention on television and radio and in newspapers are particularly likely to come to citizens' minds shortly thereafter and thereby to enjoy enhanced impact on presidential evaluations. In contrast, topics that have been addressed only minimally in the news media are rarely primed and are therefore likely to play little if any role in presidential assessment processes (Iyengar and Kinder 1987; Iyengar et al. 1984; Krosnick and Kinder 1990).

In light of these principles, it is interesting to consider the potential impact of news coverage of the 1991 Gulf War on American's evaluations of George Bush. As Figure 2 illustrates, with the onset of the war came a dramatic alteration in the content of news coverage across the country. Before the overt military actions began in Kuwait, Americans heard about events unfolding there with increasing frequency. But once the armed combat began in January, there was a significant surge in coverage (a doubling, according to the data in Figure 2). The conflict became the central focus of the media every day for months. Certainly, then, the ingredients of Americans' presidential evaluations must have shifted significantly as the result of the war if the theory of priming is correct.

As easy as this prediction is to derive from the theory, it is quite a bit more difficult to specify a priori exactly how these ingredients are likely to have shifted. The most straightforward prediction would suggest that assessments of Bush's handling of the Gulf crisis would come to loom large in people's overall evaluations of him. But it is conceivable that focusing Americans' attention on this conflict overseas might have increased attention to other political attitudes as well. That is, when a war occurs and news coverage of it is intense, it may stimulate thinking about many related issues, because the conduct of the war has effects on these issues. At the very least, because the Gulf War involved complex coordination of efforts by many countries worldwide, Americans might have become more attentive to and concerned about U.S. relations with these various other countries.

On the other hand, perhaps dramatically focusing people's thinking on the Gulf crisis would narrow their cognitive focus and thereby decrease thinking about other international issues. Along similar lines, concentrating media attention on a military conflict overseas and neglecting domestic issues such as the economy might cause the latter to recede in American's thinking. Thus, exactly which ingredients of performance evaluations rose and fell in importance presumably hinged upon precisely how Americans construed the meaning and significance of the Middle East conflict, as well as their cognitive capacities. Thus shedding light on these changes is one approach to gaining insight into how the public made sense of the war.

Our goal here is to do just this—to assess the impact of the Gulf crisis on the ingredients of presidential appraisals. To do so, we will analyze national panel survey data collected before and after the war. Included in both the prewar and postwar interviews that we examine were extensive batteries of questions assessing respondents' perceptions of presidential performance overall and in specific policy domains. Thus these data provide an opportunity to assess the impact of the war on presidential evaluation processes.

Identifying the Most Vulnerable Citizens

Once we have characterized shifts in determinants of presidential evaluations for the nation as a whole, we shall attempt to identify those segments of the public whose evaluation strategies were most and least influenced by the media. Our prior research on priming has indicated that political expertise or involvement may be an important moderator here (see Iyengar et al. 1984; Krosnick and Kinder 1990). While the judgments of political novices have at times appeared to be highly responsive to news media emphasis, experts have seemed to be much more immune to these pressures. Since we have seen this pattern before, we might see it again here.

However, the empirical evidence on the moderating role of political expertise is actually rather inconsistent, in part depending upon the particular operationalization of expertise employed. Although priming effects have been weaker among people highly knowledgeable about politics in some investigations, no differences between highly knowledgeable and less knowledgeable people have appeared in others, (cf. Iyengar and Kinder 1987, chap. 10; Iyengar et al. 1984; Krosnick and Kinder 1990). Furthermore, other methods of assessing political involvement (e.g., reported interest in politics, frequency of exposure to political news, and frequency of political participation) have failed to distinguish people most susceptible to priming from those resistant to media influence (Iyengar and Kinder 1987, 90–97). These latter findings are puzzling, especially because these measures typically possess a moderately positive association with political knowledge and might therefore be expected to behave similarly (see, e.g., Krosnick and Milburn 1990). Consequently, it seems sensible at this point to step back and think carefully about exactly how particular aspects of political expertise or involvement might shape presidential evaluation processes and therefore how they might moderate priming effects.

There are two principal perspectives on how aspects of political involvement might operate here. One has been employed frequently by political scientists in empirical investigations of American public opinion since the 1950s. The second is suggested by contemporary work in cognitive and social psychology on memory and information processing. Although these two perspectives make quite different predictions, they both share a central underlying assumption: although positively correlated with one another, different dimensions of political involvement should have opposite effects on priming, so multivariate analyses must be conducted in order to see these effects clearly.

The Traditional Dosage–Resistance Perspective. The most traditional and long-standing approach to political involvement presumes that one can understand the impact of the news media on any particular individual by considering two regulatory factors: dosage and resistance (Converse 1962; McGuire 1968; Zaller 1987, 1989, 1992). The greater one's dosage of media content, the more one should be influenced by it, whereas the more knowledge one has, the more resistant one should be. Individuals with little knowledge about politics have only a minimal ability to see flaws or distortions in new information; and they have few other bases from which to derive political judgments. Therefore, they should be highly responsive to news media content. In contrast, highly knowledgeable individuals are equipped to counterargue new information and assimilate it to their preexisting store of knowledge. They may therefore be especially resistant to the influence of any single dose of new information.

A Contemporary Psychological Perspective. Social and cognitive psychology suggest a second perspective that views the likely effects of exposure, interest, and knowledge quite differently. With regard to exposure and interest, there are at least two reasons to expect weaker priming effects among the most exposed and interested. First, individuals who are only minimally exposed to and interested in the news media will probably absorb little other than the "big message" contained in the lead stories and repeated regularly across media and across time. In contrast, highly exposed and interested individuals are likely to absorb both the "big message" and a wide range of peripheral details and additional stories. Consequently, the priming impact of the principal message is likely to be diluted by the many other knowledge domains that are also primed. Thus, priming effects of a principal story (such as the Gulf crisis) should be strongest among individuals with lower exposure and interest levels.

There is a second reason to expect this pattern, as well (see Hastie and Park 1986; Lodge, McGraw, and Stroh 1989). People who are highly attentive to the flow of political news are likely to think a great deal about political affairs. This extensive thought is likely to lead them to form a set of general political evaluations (e.g., regarding a president's overall job performance) stored in memory and to update them continually as new relevant information is acquired. Therefore, if asked by a pollster to report an assessment of a president's performance, these individuals need only retrieve and report these on-line evaluations. These judgments have a great deal of inertia, because they are based on large sets of previously acquired information, so new pieces of information have only small impact on them. Therefore, recent news media content would be expected to have relatively little effect on highly attentive citizens.

In contrast, citizens who pay little attention to the flow of news are unlikely to have such general political evaluations stored in memory. Consequently, they will most likely have to respond to an inquiring pollster by recalling whatever they can about presidential performance and deriving an overall evaluation on the spot. As the theory of priming suggests, instead of drawing upon expansive arrays of performance domains, these individuals are most likely to consider only the small handfuls of performance domains that come to mind most easily. Thus, lower levels of exposure and interest would again be associated with the greatest priming effects because of the employment of a memory-based judgment strategy.

The memory-based judgments of low-exposure, low-interest individuals will only be influenced by news media content absorbed days or weeks earlier if that content had been stored in their memories and could be retrieved at the time of judgment. Therefore, factors that enhance the likelihood of storage and retrieval will enhance media priming effects. One such factor is political knowledge. The more schematically organized knowledge one has about a domain, the more efficiently and effectively one can make sense of new information, find an appropriate place to incorporate it into one's memory, and retrieve it sometime later (see Fiske and Taylor 1991, 121–32). Therefore, the more knowledge one has about politics, the more quickly and easily one can make sense of a news story and the more efficiently one can store it in, and retrieve it from, an elaborate and organized mental filing system. In contrast, individuals with minimal knowledge about politics presumably have more difficulty interpreting new political information, are unlikely to store that information in memory, and have difficulty retrieving whatever information they do store. Therefore, media priming effects would presumably be greater among highly knowledgeable individuals than among minimally knowledgeable ones.

The Present Investigation

Exposure, interest, and knowledge are typically positively correlated with one another, but these two theoretical perspectives predict these variables to have oppositely signed effects on priming. Therefore, these perspectives offer potential explanations for the inconsistencies in prior studies of priming. None of these studies entailed multivariate analyses gauging the effect of each dimension of involvement while controlling for the effects of the others. Therefore, the resulting confounding of oppositely signed effects could well have yielded inconsistencies from study to study. All of this suggests that we should conduct multivariate analyses and look for additive and interactive effects of political involvement measures on changes in presidential evaluation ingredients produced by the Gulf War.

DATA

The data that we analyzed in this study were collected as a part of the 1990–91 National Election Panel Study (NES) of the Political Consequences of War. For this survey, a nationally representative sample of 2,000 American adults was interviewed during November and December 1990 and January 1991, prior to the major events of the war and the dramatic increase in media attention to the Gulf crisis. Of these individuals, 1,385 were interviewed a second time during June and July 1991, long after the military efforts were completed.

During both the prewar and postwar interviews, respondents were asked extensive series of questions addressing many political beliefs, attitudes, and behaviors. Of most significance for the current investigation, respondents appraised George Bush's handling of the Persian Gulf crisis, his handling of foreign affairs generally, his handling of the domestic economy, and his job performance in general. Also, respondents reported their overall evaluations of Bush on a 101-point feeling thermometer. We combined these latter two judgments into a single index of respondents' attitudes toward Bush by averaging them together after recoding both sets of responses to range from 0 to 1.[1]

In addition, respondents were asked questions measuring their knowledge about politics in general, their exposure to political information through the news media, and their interest in the Gulf War. We gauged political knowledge by the number of correct answers respondents gave to nine questions asking about factual matters, such as what political office Dan Quayle held and which party had the most members in the Senate. Media exposure was assessed by an index that combined answers to questions about frequency of watching television news in general, watching television news stories about the 1990 elections, reading newspapers in general, and reading stories in newspapers about the 1990 elections. Interest in the Gulf War was assessed by a single question asking respondents how much attention they paid to news about the war. As expected, these three indices of political involvement were moderately positively correlated with one another ($r_{\text{knowledge, media}} = .35, p < .01; r_{\text{knowledge, interest}} = .18, p < .01; r_{\text{media, interest}} = .29, p < .01$).

Our analyses focussed on only those respondents who answered all of these questions, a total of 1,090. These individuals constitute 78.7% of the total panel sample and 54.5% of the total wave 1 representative national sample of 2,000 people. As the Appendix illustrates, the subsample of 1,090 individuals is quite similar to the full representative sample of 2,000 respondents in terms of demographic characteristics (gender, race, age, educational attainment, and region of residence) and general political judgments (Bush approval ratings and party identification). Thus, we see little reason to worry about the subsample's representativeness.

Table 1 displays the prewar and postwar distributions of the subsample's responses to the Bush evaluation questions.[2] With regard to overall attitudes toward Bush, we see a sharp increase in approval after the war that matches the increase in support illustrated in Figure 1. Our sample also voiced more approval of Bush's handling of the Gulf crisis, foreign relations, and the economy after the war than before. Thus, the war clearly enhanced Americans' sentiments toward Bush. We turn now to assessing whether the war also altered the relations between these various judgments.

RESULTS

We assessed the impact of the war on the ingredients of overall Bush evaluations by specifying the following regression equation:[3]

TABLE 1

Presidential Evaluation before and after the War (%)

EVALUATION	PREWAR	POSTWAR
Bush's job performance		
Approve strongly	30.2	50.3
Approve not strongly	36.2	29.6
Disapprove not strongly	13.8	7.9
Disapprove strongly	19.8	12.2
Feelings toward Bush		
0–25	8.1	6.8
26–50	19.7	16.2
51–75	40.6	26.4
76–100	31.6	50.6
Bush's handling of		
The Persian Gulf crisis		
Approve strongly	38.3	67.3
Approve not strongly	23.5	17.2
Disapprove not strongly	12.6	4.5
Disapprove strongly	25.6	10.9
Foreign relations		
Approve strongly	42.5	55.2
Approve not strongly	24.0	23.8
Disapprove not strongly	8.6	6.2
Disapprove strongly	24.9	14.8
The economy		
Approve strongly	15.9	21.4
Approve not strongly	27.9	33.3
Disapprove not strongly	19.6	17.2
Disapprove strongly	36.6	28.1

Note: Number of cases = 1,090.

$$\text{Overall attitude} = b_0 + b_1\,(\text{economy approval}) + b_2\,(\text{foreign relations approval}) + b_3\,(\text{Gulf crisis approval}). \quad (1)$$

To examine shifts in ingredients for the full sample, we estimated equation 1 twice, once using the prewar judgments and once using the postwar judgments. (All variables were coded to range from 0 to 1, with larger values indicating more positive evaluations.) We expected that b_3 would increase after the war, suggesting a priming effect. If the news media's attention to the Gulf crisis led Americans to focus their thinking on that incident to the exclusion of other foreign relations issues, b_2 should decline. If, on the other hand, media attention to the Gulf crisis led Americans to think more about other foreign relations matters, b_2 should increase. Finally, because increased attention to the Gulf crisis presumably pushed domestic economic problems out of the spotlight, we expected that b_1 would decline after the war.

Equation 1 was estimated via ordinary least squares regression, and the resulting unstandardized coefficients are displayed in Table 2.[4] As expected, the impact of Gulf crisis performance on overall evaluations increased from .21 to .30, a 50% jump ($z = 3.30$, $p < .001$). The impact of foreign relations performance decreased slightly, and performance in handling economic issues had slightly greater impact after the war, though these differences were nonsignificant. Thus, it seems that for the nation as a whole, news media attention to the war increased the impact of Gulf crisis performance on overall Bush evaluations without shifting the impact of other ingredients.

Next, we estimated equation 1 separately for respondents high and low in knowledge, media exposure, and interest (see Table 3).[5] Despite the relative independence of the three involvement factors from one another, the results are largely comparable for all of them. First, the increase in impact of Gulf performance is pronounced and statistically significant for the low-involvement group but not for the high-involvement group in each case. Furthermore, although no significant changes in the impact of foreign relations and economic performance appear, trends suggest that their impact may have increased after the war in the high-involvement groups and decreased after the war in the low-involvement groups.

In order to test the differences between the high- and low-involvement groups formally, we analyzed both the prewar and postwar data simultaneously in a within-subjects repeated-measures regression. In addition to the predictors in equation 1, we also included a dummy variable for time (coded 0 for the prewar data, 1 for the postwar data) and interactions between this dummy variable and the three domain-specific performance appraisals. The statistical significance of these interactions assess the robustness of the differences between the prewar and postwar coefficient for each of the performance predictors. We also included a main effect of political involvement (either knowledge, exposure, or interest) and all possible interactions between involvement and the other variables in the equation.[6] This equation was estimated three times, once for each of the three involvement factors.

Of most interest are the three-way interactions between the domain-specific performance measures, time, and the involvement measures (see Table 4). Surprisingly, none of the interactions involving political knowledge are significant. However, for exposure and interest, all six interactions are significant or marginally significant. Thus, the impact of Gulf crisis performance increased more among the low-exposure and low-interest groups than among the high-exposure and high-interest groups. And whereas the impact of economic and foreign relations performance increased in the high-exposure and high-involvement groups, their impact decreased in the low-exposure and low-involvement groups.[7]

All of the results thus far are consistent with previous research on priming and with reasonable expectations about the Gulf crisis. Among people minimally involved in politics, the Gulf crisis became a more important basis of presidential evaluations after the war, while the impact of foreign affairs and the domestic economy decreased. In contrast, among people highly engaged by politics, the ingredients of presidential evaluations were essentially unaltered. When our three measures of political involvement

TABLE 2

Estimates of the Impact of Domain-specific Evaluations on Overall Evaluations of Bush's Performance before and after the War

PERFORMANCE DOMAIN	UNSTAND. REGRESS. COEFF. PREWAR	UNSTAND. REGRESS. COEFF. POSTWAR	PRE–POST DIFFERENCE	Z-STAT. TESTING SIGNIF. OF THE DIFF.
Gulf crisis	.21* (.02)	.30* (.02)	.09	3.30*
Foreign relations	.22* (.02)	.21* (.02)	−.01	−.31
Economy	.26* (.02)	.27* (.02)	.01	.18

Note: Number of cases = 1,090. Standard errors are shown in parentheses. Assessments of President Bush's Gulf crisis, foreign relations, and economy performance were the only predictors in these equations.
*$p < .001$.

were considered individually, media exposure and interest were the most effective at illustrating differences between high- and low-involvement individuals, whereas political knowledge was least effective at doing so (a result that conflicts with patterns observed by Zaller [1990] in similar head-to-head comparisons involving phenomena other than priming).

However, as we have suggested, considering knowledge, exposure, and interest individually may not be the most sensible approach. We therefore estimated a much-expanded within-subjects repeated-measures regression equation that again predicted overall Bush evaluations including all three aspects of political involvement simultaneously. The main effect variables included here were Gulf crisis performance, foreign relations performance, economic performance, time (prewar vs. postwar), knowledge, exposure, and interest. All possible two-way and three-way interactions among these variables were included, as were three four-way interactions: time × interest × exposure × each of the three domain-specific performance appraisals. Most of the terms in this huge regression are of no interest theoretically in the present context, but a subset of them tests specific hypotheses.

First, we expected that the main effects of Gulf crisis performance, foreign relations performance, and economic performance would all be positive and significant, indicating that overall evaluations of Bush at the prewar time point were based partly on each of these three ingredients. The basic priming effects (among individuals low in knowledge, exposure, and interest) are tested by the two-way interactions between time and the three domain-specific performance assessments. We expected that these interactions would be positive for Gulf crisis performance (indicating increased impact) and negative for foreign relations and economic performance (indicating decreased impact over time).

The three-way interactions between time, domain-specific performance, and each of the three involvement measures test whether the increases and decreases in domain-specific performance impact are different in the high involvement groups. If the traditional dosage–resistance model is correct, then the three-way interactions involving exposure and interest should have the same algebraic signs as the two-way interactions estimating the basic priming effects—positive for Gulf crisis performance and negative for foreign relations and economic performance (indicating greater priming at higher exposure and interest). For knowledge, the three-way interactions should have the opposite algebraic signs—negative for Gulf crisis performance and positive for foreign relations and economic performance (indicating decreased priming at high knowledge). In contrast, the information-processing perspective predicts the opposite effects: for exposure and interest, it predicts a negative sign for Gulf crisis performance and positive signs for foreign relations and economic performance; for knowledge it predicts a positive sign for Gulf crisis performance and negative signs for foreign relations and economic performance.

These predictions regarding the three-way interactions treat exposure and interest as additive components of dosage or information-processing load. But in fact, they are likely to be interactive. That is, if an individual is completely uninterested in politics, then increased exposure to the news media should have little or no effect on impact. Thus only exposure in the presence of interest should be consequential. Therefore, the four-way interactions between time, domain-specific performance, exposure, and interest should be significant and should have the same algebraic signs as the basic priming effects: according to the traditional perspective, positive for Gulf crisis performance and negative for foreign relations and economic performance; according to the information-processing perspective, negative for Gulf crisis performance and positive for foreign relations and economic performance.

The relevant unstandardized regression coefficient estimates we obtained are displayed in Table 5.[8] The top third of the table addresses changes in the impact of Gulf crisis performance assessments. Prior to the war, these assessments had no impact on overall

TABLE 3

Estimates of the Impact of Domain-specific Evaluations on Overall Evaluations of Bush's Performance before and after the War, Separately for High- and Low-Political-Involvement Groups

INVOLVEMENT GROUP[a] AND PERFORMANCE DOMAIN	UNSTAND. REGRESS. COEFF.***		PRE–POST DIFFERENCE	Z-STAT. TESTING SIGNIF. OF THE DIFF.
	PREWAR	POSTWAR		
Knowledge				
Low (N = 688)				
Gulf crisis	.20 (.02)	.30 (.03)	.10	2.95**
Foreign relations	.21 (.02)	.18 (.02)	−.03	−.97
Economy	.25 (.02)	.24 (.02)	−.01	−.42
High (N = 402)				
Gulf crisis	.23 (.03)	.29 (.03)	.06	1.27
Foreign relations	.25 (.03)	.27 (.03)	.02	.66
Economy	.28 (.03)	.31 (.03)	.03	.72
Exposure				
Low (N = 554)				
Gulf crisis	.21 (.03)	.36 (.03)	.15	4.00***
Foreign relations	.20 (.03)	.15 (.03)	−.05	−1.25
Economy	.25 (.03)	.23 (.02)	−.02	−.41
High (N = 536)				
Gulf crisis	.22 (.03)	.26 (.03)	.04	1.01
Foreign relations	.24 (.03)	.25 (.03)	.01	.42
Economy	.28 (.02)	.29 (.02)	.01	.62
Interest				
Low (N = 379)				
Gulf crisis	.19 (.04)	.39 (.03)	.20	4.18***
Foreign relations	.22 (.04)	.15 (.03)	−.07	−1.41
Economy	.29 (.03)	.21 (.03)	−.08	−1.82*
High (N = 711)				
Gulf crisis	.22 (.02)	.26 (.02)	.04	1.10
Foreign relations	.22 (.02)	.24 (.02)	.02	.66
Economy	.26 (.02)	.30 (.02)	.04	1.54

Note: Standard errors of the coefficients are shown in parentheses. Assessments of President Bush's Gulf crisis, foreign relations, and economy performance were the only predictors in these equations, which were estimated separately for each involvement group. All unstandardized regression coefficients are significant at .001 level.

[a] Knowledge was gauged by the number of correct answers that respondents gave to nine questions asking about factual matters. Exposure was assessed by questions about frequency of exposure to television news and newspapers. Interest was assessed by a question gauging attention respondents paid to news about the war.

*$p < .10$.

**$p < .01$.

***$p < .001$.

TABLE 4

Estimates of the Impact of Political Involvement Factors in Regulating the Priming Effect

INVOLVEMENT DIMENSION AND INTERACTION	UNSTAND. REGRESS. COEFF.	T	*P*
Knowledge × time ×			
Gulf crisis	.15 (.12)	1.26	.21
Foreign relations	−.01 (.12)	.08	.93
Economy	−.04 (.10)	.39	.70
Exposure × time ×			
Gulf crisis	−.33 (.10)	3.16	.002
Foreign relations	.18 (.10)	1.75	.08
Economy	.17 (.09)	1.91	.06
Interest × time ×			
Gulf crisis	−.43 (.14)	3.02	.003
Foreign relations	.34 (.14)	2.50	.01
Economy	.22 (.13)	1.77	.08

Note: Number of cases = 1,090. Standard errors of the coefficients are shown in parentheses. Separate regressions were performed for each measure of involvement (knowledge, exposure, and interest). In each analysis, Gulf crisis performance, foreign relations performance, economy performance, involvement (either knowledge, exposure, or interest), and a dummy variable for time (coded 0 for prewar data and 1 for postwar data) were entered as main effects, in addition to all two-way interactions and three three-way interactions between involvement (knowledge, exposure, or interest), domain-specific performance (Gulf crisis, foreign relations, or economy), and time (prewar vs. postwar).

TABLE 5

Estimaters of Main Effects and Interactions Predicting Overall Evaluations of George Bush Using Domain-specific Performance Evaluations and Political Involvement Factors

PREDICTORS	UNSTAND. REGRESS. COEFF.	T	P
Gulf Crisis	−.27 (.25)	1.10	.27
Gulf crisis × time	.88 (.29)	3.01	.003
Gulf crisis × time ×			
Knowledge	.41 (.13)	3.18	.002
Exposure	−1.35 (.54)	2.51	.012
Interest	−.94 (.34)	2.81	.005
Exposure × interest	−1.10 (.59)	1.86	.063
Foreign relations	.64 (.28)	2.26	.024
Foreign rel. × time	−.36 (.29)	1.26	.21
Foreign rel. × time ×			
Knowledge	−.25 (.13)	1.91	.056
Exposure	.52 (.52)	1.00	.319
Interest	.48 (.33)	1.48	.14
Exposure × interest	−.37 (.57)	0.64	.52
Economy	.46 (.24)	1.97	.05
Economy × time	−.51 (.27)	1.93	.054
Economy × time ×			
Knowledge	−.12 (.11)	1.08	.28
Exposure	.96 (.45)	2.11	.035
Interest	.72 (.30)	2.36	.018
Exposure × interest	−.91 (.50)	1.82	.069

Note: Number of cases = 1,090. Standard errors of the coefficients are shown in parentheses. In this regression, the following predictors were entered as main effects: Gulf crisis performance, foreign relations performance, economy performance, knowledge, exposure, interest, and time (prewar vs. postwar). Also entered were all two-way interactions, all three-way interactions, and three four-way interactions between time, exposure, interest, and domain-specific performance (Gulf crisis, foreign relations, or economy).

evaluations of Bush ($b = .27$, n.s.). But after the war, the impact of these assessments became strong and significant among low involvement individuals ($b = .88$, $p < .003$), indicating a typical priming effect. Knowledge, exposure, and interest all regulated the magnitude of this priming effect but in directions opposite to the traditional view and consistent with the information-processing view. The priming effect became *stronger* as political knowledge increased ($b = .41$, $p < .002$) and *weaker* as exposure increased ($b = -1.35$, $p < .01$) and as interest increased ($b = -.94$, $p < .005$). The exposure × interest interaction indicates that the additive effects of exposure and interest did not combine fully linearly; rather, the priming effect is a bit stronger at the highest levels of exposure and interest than would be expected based on the main effects alone ($b = 1.10$, $p < .07$).

With regard to assessments of foreign relations performance (as shown in the middle third of Table 5), the pattern is the same, though weaker in statistical terms. Overall Bush evaluations were partly based on assessments of his handling of aspects of foreign affairs other than the Gulf crisis before the war ($b = .64$, $p < .03$). This impact tended to decline a bit (although not significantly) among low-involvement individuals after the war ($b = -.36$, $p < .21$), but it declined more among respondents high in political knowledge ($b = -.25$, $p < .06$). Though not significantly, high levels of exposure and interest were again associated with reduced priming effects (b = .52, p = .32 and b = .48, p = .14, respectively). Exposure coupled with interest was again associated with a slightly strengthened priming effect ($b = -.37$, p = .52), though this effect is not even close to being statistically significant.

More statistically significant coefficients emerged for economic judgments, as displayed in the bottom third of Table 5. Overall assessments of Bush were based in part on assessments of his economic performance before the war ($b = .46$, $p < .05$). And after the war, the impact of economic judgments was decreased significantly ($b = -.51$, $p < .06$), presumably reflecting the news media's dominant focus on the war. Although not statistically significantly, high levels of knowledge were again associated with an enhanced priming effect ($b = -.12$, $p = .28$). And the priming effect was again stronger at higher levels of exposure ($b = .96$, $p < .04$) and interest ($b = .72$, $p < .02$). As before, the priming effect was strengthened when both exposure and interest were coupled ($b = -.91$, $p < .07$).

These results make it clear that priming affected the impact of all three policy domains on overall presidential evaluations. Furthermore, these effects were clearly regulated by the three dimensions of political involvement in different ways. Thus the findings just described highlight significant psychological shifts in the American public's political information processing. In order to understand the political significance of these effects, however, we must assess how much of the postwar rise in President Bush's approval ratings is attributable to these priming effects.

As Figure 1 illustrates, presidential approval increased from 56% in October–November 1990 to 76% in June 1991. Reflecting this surge, our sample's mean overall presidential evaluation increased from .613 before the war to .722 afterward, a change of .109 units on the 0 to 1 scale. This increase is presumably due to at least two changes: increased positivity of Americans' assessments of President Bush's handling of the Gulf crisis and his handling of foreign relations (as shown in Table 1) and increased weight attached to these two domains and reduced weight attached to his handling of the economy, which was evaluated more negatively.

To assess how much of the increase in overall evaluations was due to each of these components, we used the regression equation described in Table 5 to estimate what postwar evaluations of Bush's overall performance would have been had the ingredient weights remained unchanged. This exercise yielded a predicted mean overall evaluation of .702, .089 units higher than the prewar average evaluation. Thus, the shift in domain-specific ratings could account for 82% of the pre-to-post increase. Substituting the postwar weights to generate predicted overall evaluations increased the mean by .020 units, or 18% of the total pre-to-post surge. Thus, of the 20% increase from prewar to postwar shown in Figure 1, almost four of these percentage points are attributable to priming.

DISCUSSION

The big news here is in two parts: the impact of the Gulf War on Americans' evaluations of their president's performance and the roles of political knowledge, exposure, and interest in regulating priming effects.

The Impact of the Gulf Crisis

Consistent with previous research on priming, the news media's incessant focus on the Gulf crisis appears to have increased the impact of Americans' assessments of George Bush's handling of it on their evaluations of his overall job performance. And at the same time, attention to the war apparently reduced attentiveness to other aspects of foreign relations and to the domestic economy.

Because most Americans evaluated Bush's handling of the Gulf crisis much more positively than they evaluated his handling of the domestic economy or other aspects of international relations, this shift in focus may have contributed to the sharp increase in Bush's overall popularity illustrated in Figure 1. Of course, simply "winning" the war was, in and of itself, probably largely responsible for the surge in approval ratings. But priming seems to have contributed significantly to this shift as well.

These findings are in marked contrast to one plausible vision of the American citizen. One might imagine that political affairs are at the periphery of most Americans' lives most of the time because the more mundane and personal concerns of everyday life exhaust their abilities for cognitive engagement. But when a war occurs and intrudes into the daily lives of numerous people in many powerful ways, their interest in and exposure to political affairs presumably surges. It is conceivable that this shift might lead people to become thoughtful about a wide range of political issues indirectly related to the war. Thus, perhaps ordinarily passive citizens are drawn into political affairs when their attentions are grabbed by large-scale and consequential events such as wars.

In fact, our results offer no support for this vision. Americans were apparently quite narrow in their response to the war, focusing their attentions on it to the exclusion of other issues, even other foreign relations issues. Thus, the Gulf War did not induce cognitive engagement in an array of political issues. This result is in harmony with evidence from laboratory experimental studies showing that priming effects appeared only in policy domains that have been directly addressed in news coverage (see Iyengar and Kinder 1987, chap. 11). This correspondence enhances our confidence in those studies and in ours.

Might other wars have been capable of inciting broad cognitive engagement in politics? Certainly, the Gulf War was unusual in many ways, in terms of minimal casualties, brief combat engagement, and a variety of other attributes. Perhaps more costly wars conducted over more protracted periods of time lead Americans to attach cognitive significance to many other policy issues. But it may instead be that Americans' capacities for attention are limited to just a few big issues at a time.

Moderators of Priming

When considered individually, political knowledge, exposure, and interest all seemed to have the same effects on priming: higher involvement was associated with reduced priming, just as has been seen in various previous investigations (Iyengar et al. 1984; Krosnick and Kinder 1990). However, when these three variables were examined simultaneously, they turned out to have opposite effects on priming. High levels of political knowledge enhanced priming, and high levels of exposure and interest reduced priming, although the coupling of exposure and interest was associated with slightly increased priming.

The directions of these effects are in sharp contrast to those anticipated by a traditional view of political involvement. Greater media exposure and interest have been viewed as indicating stronger dosages of media content, whereas greater knowledge has been viewed as a basis for greater resistance to the new information acquired from the media. Our findings clearly challenge this view and instead support the contemporary psychological alternative. Greater knowledge constitutes a greater ability to interpret, encode, store, and retrieve new information. And higher levels of exposure and interest are associated with a greater likelihood of forming on-line political evaluations and a dilution of priming effects due to a wider range of knowledge being activated by media coverage.

There is another way to think about the effect of political interest that we identified. Recent studies suggest that priming manipulations may have the greatest impact when they occur without people paying much attention to them (Lombardi, Higgins, and Bargh 1987; Strack, Schwarz, Bless, Kubler, and Wanke 1993). When people attend closely to and are aware of the potential impact of context on their judgments, they correct for it. In our case, highly interested viewers may therefore have said to themselves, "I know the news media have paid a lot of attention to inflation lately, but I don't want that to cause me to place too much weight on inflation in deriving my presidential performance evaluation. So I'll reduce its weight a bit." This could explain why weaker priming effects appeared among the most interested readers and viewers.

It is important to acknowledge, however, that the exposure and interest effects we observed could have occurred at a point in time much later than encoding, via retroactive interference. People who were especially interested in politics and who absorbed lots of information about the war no doubt continued to attend closely to information about politics after the war as well. Consequently, as the news media's attention to matters other than the Gulf crisis increased during April and May of 1991, these highly attentive individuals were filling their minds with this new information, which would interfere with the cognitive effects of previously stored knowledge about the war. Thus, greater interest and exposure may yield more absorption of information about the war but also more absorption of information about a range of other political matters as well. Individuals who were typically only minimally interested in and exposed to political information would have absorbed little political news after the war and would therefore have experienced much less postwar interference. This may explain why these people based their presidential evaluations in June and July 1991 on war-related attitudes.

This alternative interpretation of our results makes it clear how the timing of survey interviews can have tremendous impact on substantive conclusions about the war. Had a set of interviews been conducted during the war, in late January or February, we could have assessed the impact of exposure and interest on priming while avoiding confounding these factors with retention or retroactive interference. We might, therefore, have found more priming to be associated with greater exposure and interest at that time, reflecting greater dosage of media content, just as the traditional perspective on these measures of political involvement would have anticipated. In the absence of such data, the plausibility of retroactive interference must remain unclear for the moment.

Our results regarding knowledge, exposure, and interest do not challenge the *assumptions* underlying the dosage–resistance perspective. There is enough other available evidence supporting these notions in general (see Petty and Cacioppo 1986; Zaller 1992) that they need not be called into question here. Rather than challenging these general perspectives, our results challenge the *application* of this perspective to news media priming. That is, the failure of knowledge, exposure, and interest to behave like dosage and resistance factors suggests that priming does not entail a persuasionlike process of belief shifts but instead occurs by shifts in decision-making *criteria*.

Generalizing to the Iran–Contra Affair

The theoretical significance of these findings regarding political involvement is a bit difficult to assess with just this one investigation in hand. It is conceivable that there is something unique about the Gulf War that is responsible for these effects, so we might observe different patterns of results for other events. On the other hand, it is also possible that the patterns of effects observed here do indeed generalize across political contexts.

To assess this possibility, we conducted a reanalysis of data regarding priming in the context of the Iran–Contra affair from a study that ignored exposure and interest and examined only the effect of political knowledge and, as expected, found priming to be decreased among highly knowledgeable citizens (Krosnick and Kinder 1990). Using the same methodology, we returned to those data and assessed the simultaneous effects of knowledge, exposure, and interest. Our findings were consistent with the ones we have reported regarding exposure and interest in the Gulf War context. High levels of exposure and interest were each associated with *reduced* priming effects individually (for exposure, $t = 1.86$, $p = .064$; for interest, $t = 1.67$, $p = .096$; $N = 767$); and the confluence of exposure and interest slightly decreased their individual impacts ($t = 1.84$, $p = .066$). Once we controlled for these effects, political knowledge had no impact on priming ($t = .79$, $p = .43$).

These findings are encouraging in two regards. First, they suggest that the negative impact of exposure and interest on priming is not limited to the Gulf War case. In addition, this replication suggests that our Gulf War results are not the artifactual result of aspects of the 1990–91 NES panel study design, because the Iran–Contra study design is quite different in numerous ways. For example, instead of being a panel study, the Iran–Contra investigation involved two independent cross sections of respondents: one interviewed before the scandal was revealed, and the other afterward. And instead of a delay of months between the enhanced media coverage and postevent interviews, the Iran–Contra postevent interviews were conducted beginning the day after the revelation. It therefore seems unlikely that our present results are artifactually produced by some aspect of the 1990–91 NES study design. Furthermore, the retroactive-interference explanation for the interest and exposure effects we have described seems unlikely as well.

It is conceivable, however, that study design differences may account for the disappearance of the knowledge effect in the Iran–Contra case. Recall that knowledge enhances priming by facilitating information storage and retrieval processes. Such processes are very likely to be involved in long-term priming over a period of months, as was the case in the Gulf War. But because the postevent interviews for the Iran–Contra case were conducted immediately after the revelation, long-term storage and retrieval issues were probably less relevant, thus explaining the lack of a knowledge effect in this case.

Alternative Explanations for Our Findings

We have presumed all along that the shifts in the ingredients of presidential evaluations observed here are due to the increase in news media attention to events unfolding in the Gulf. But of course, we have provided no definitive evidence to support this causal assertion. And other theoretical perspectives would attribute the surge in presidential support to other causes. For example, Brody (1991) has argued that presidents enjoy enhanced popularity immediately after bold internation acts because their critics are temporarily quiet. Sigelman and Conover (1981) have argued that Americans draw together in response to the external threat that war represents, and positive feelings about the nation and its leader swell. (For a review of other possible alternative explanations, see Brody 1991.)

It is certainly conceivable that these or other explanations could be partly or even fully responsible for President Bush's increased public support immediately after he initiated military action in the Gulf. Unfortunately, the data we have at hand do not allow us to test these alternative explanations or to pit them against the account of these events based on priming. For the moment, then, priming must remain a viable explanation, though not necessarily the definitive one.

Dimensions of Political Involvement

Finally, our results suggest that researchers studying public opinion take a step back from traditional views of political involvement in light of contemporary theories of information processing and decision making. First, the sometimes-employed practice of combining measures of political knowledge, interest, and exposure into a single index of political involvement (e.g., Cassel 1984; Chong, McClosky, and Zaller 1983; Judd and Downing 1990) now seems especially unwise, given that it may obscure unique effects of the components. Furthermore, head-to-head comparisons looking for the single most effective measure of involvement (e.g., Zaller 1990) also seem misdirected, because different dimensions may have oppositely signed effects and therefore do not reflect a single underlying construct. In addition, it seems risky to examine the impact of only a single dimension, such as exposure (see, e.g., Bartels 1993), given that controlling for other dimensions, such as interest and knowledge, can dramatically change that first dimension's apparent effect. Instead, our results (like other recent investigations, e.g., Krosnick and Milburn 1990), illustrate why it is most sensible to treat political knowledge, interest, and exposure as distinct constructs that may have distinct effects on political information processing and decision making. Multivariate analyses simultaneously examining these three dimensions clearly seem in order for future studies.

This conclusion has substantive as well as methodological implications. Theorists have catalogued the many benefits of mass public engagement in the political affairs of democracies, as well as the dangers of apathy and ignorance (e.g., Dahl 1989). At the same time, political observers have often lamented lack of political involvement in the American public and other mass publics across the globe (e.g., Kinder and Sears 1985). The operating assumption here is that political involvement is a single entity that can be maximal, moderate, or minimal for a nation as a whole or for any given individual. This assumption leads observers to presume that any single indicator of low involvement (such as a low turnout rate in an election) reflects low levels of engagement generally (e.g., Milbrath 1965).

Our results suggest a different view of political involvement, one that treats involvement as having multiple distinct components. Any given individual may be exposed to lots of political information but may have no interest in it and may therefore retain little of it. Alternatively, although an individual may be very interested in politics, other aspects of life may be extraordinarily demanding, leaving little time to attend to contemporary events. Or, by virtue of living in a household with others who frequently discuss political affairs, one may become highly knowledgeable about politics without any interest or exposure to the news media.

This new perspective is both encouraging and discouraging about the prospects for increasing mass involvement in democracies. On the bright side, any single piece of evidence indicating mass disengagement should not be so disappointing. That is, political involvement may be higher at a particular point in history than any single indicator (e.g., voter turnout) might suggest. But on the more discouraging side, the task of increasing political involvement entails increasing many different and distinct components: interest in political affairs, exposure to political information, storage of that information in memory, behavioral participation in political events, and so on. Our results suggest that large-scale social interventions intended to maximize involvement must be multipronged and therefore perhaps much more complex and expensive than a simpler view of involvement might support. This is not to say that such efforts will be futile but rather simply to suggest why they may be more difficult than might be imagined and how to make them most effective.

CONCLUSION

In reporting our most recent investigation of priming effects, we concluded that "change over time in popular approval [of presidential performance]—and thus the waxing and waning of presidential power—may depend the most on the citizens who know the least" (Krosnick and Kinder 1990, p. 510). The findings reported here clearly demand a revision of this conclusion. Instead, media-priming-induced shifts in presidential popularity and power seem to be driven by citizens who are highly knowledgeable about politics but relatively inattentive to the flow of political news. This more textured conclusion makes it clear that in political science, as in all of life, things are often more complex than they at first appear.

APPENDIX

Table A-1 compares demographic characteristics and general political judgments of the original representative national sample of 2,000 and the subsample of 1,090 who completed all questions that we used for our analysis. The subsample is seen to be quite similar to the parent sample.

TABLE A-1

Characteristics of the Full Representative National Sample and the Subsample Analyzed (%)

CHARACTERISTIC	SUBSAMPLE WITH COMPL. DATA	COMPL. REPRESENT. NAT. SAMPLE
Gender		
Male	48.0	45.2
Female	52.0	54.8
Race		
While	87.7	83.4
Black	10.0	13.6
Others[a]	2.3	2.7
Age		
18–29	22.7	23.5
30–39	26.0	23.1
40–49	19.1	16.7
50–59	10.8	11.2
60–69	11.8	12.0
70–94	9.9	13.5
Education		
8 grades or less	4.9	9.6
9–11 grades	11.2	14.1
High school diploma	36.1	35.2
Some college	17.5	15.3
Junior or comm. coll. degree	6.7	6.3
B.A.-level degree	16.0	12.8
Advanced degree	7.6	6.2
Region		
Northeast	20.5	20.2
North Central	28.3	25.9
South	29.6	34.6
West	21.7	19.3
Bush's Job Performance		
Approve strongly	30.2	28.6
Approve not strongly	36.2	36.2
Disapprove not strongly	13.8	15.2
Disapprove strongly	19.8	20.1
Party Identification		
Republication	27.8	24.1
Democrat	38.4	39.0
Independent	28.9	28.4
Other (no preference, don't know)	4.9	8.3

Note: Number of Cases = 1,090.

[a] American Indian, Alaskan, Asian, or Pacific Islander.

Notes

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1. We obtained similar results to those we report when we repeated our regressions using only the Bush job performance rating or the feeling thermometer rating as the dependent variable.

2. The domain-specific evaluations were moderately to strongly correlated with one another. For prewar, $r_{\text{economy, relations}} = .45, p < .01$; $r_{\text{economy, Gulf}} = .44, p < .01$; and $r_{\text{Gulf, relations}} = .70, p < .01$. For postwar, $r_{\text{economy, relations}} = .42, p < .01$; $r_{\text{economy, Gulf}} = .37, p < .01$; and $r_{\text{Gulf, relations}} = .57, p < .01$.

3. One might imagine that assessments of President Bush's foreign relations performance are based partly upon evaluations of his handling of the Gulf crisis. If so, effects of the latter in equation 1 would reflect only the direct effect (instead of the total effect) of Gulf crisis performance on overall

performance evaluations. However, when we reestimated equation 1 omitting foreign relations performance assessments, the results for Gulf crisis and economy performance were essentially identical. Therefore, indirect effects of Gulf crisis performance via foreign relations performance do not seem problematic here. Also, the results that we report were not meaningfully altered when we included controls for demographics (i.e., gender, race, age, education, and region) and partisanship in our regressions.

4. We also performed this analysis and those that follow after subjecting the dependent variable to a probit transformation. These analyses yielded results comparable to those reported below.

5. The high- and low-involvement groups were formed by selecting points near the means of the knowledge, exposure, and interest distributions that yielded approximately equal-sized high and low groups while, if necessary, making the high-involvement group a bit smaller than the low-involvement group.

6. In these equations, the political involvement indices were entered as continuous variables, rather than dichotomous ones.

7. Zaller (1992) has suggested that priming effects like these should be more pronounced among respondents moderate in political involvement than among respondents very high or very low in involvement. We found a curvilinear pattern matching this prediction when we separated respondents into low-, moderate-, and high-knowledge groups, but this pattern did not even approach statistical significance. Furthermore, purely monotonic patterns appeared in the cases of exposure and interest. We therefore did not pursue the possibility of curvilinear effects further.

8. Of the remaining 48 coefficients not shown, only 5 were statistically significant. Of these, only 3 seem theoretically meaningful: at the prewar wave, Gulf crisis performance had heightened impact on overall evaluations among individuals high in exposure or high in interest, although the conjunction of exposure and interest reduced these effects.

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CONTROVERSY

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INEQUALITY AND POLITICAL VIOLENCE REVISITED

In their 1987 article in this Review, *Muller and Seligson used logged ordinary least-squares (LOLS) to estimate the effect of income inequality on cross-national levels of deaths by political violence. T. Y. Wang challenges the robustness of the main conclusion and argues for the application of a maximum likelihood approach—the exponential Poisson regression (EPR) model—rather than LOLS. He concludes that the widely used LOLS approach yields misleading conclusions when applied to event count data. Dixon, Muller, and Seligson replicate previous work using both LOLS and EPR approaches and conclude that in most—but not all—respects the two approaches yield similar results, supporting the effect of inequality when the specifications are identical. They also argue (in response to concerns expressed by Brockett 1992) that the inequality results are robust when account is taken systematically of the best information on underreporting of deaths.*

COMMENT

Inequality in its various forms has long been regarded as a major determinant of political violence. The theoretical foundation of this argument is the relative deprivation thesis, which holds that discontent generated from the gap between an individual's expected and achieved well-being is a major cause of violent behavior (Gurr 1970).[1] A body of quantitative literature has emerged to examine the causal linkage between inequality and collective violence.[2] While different findings have been registered, most of the studies share two important methodological similarities: (1) many studies used *event count data*[3] as measurements of dependent variables with a measure of deaths from violence as the most widely used indicator (see, e.g., Boswell and Dixon 1990; Hartman and Hsiao 1988; Muller 1985, 1988b; Muller and Seligson 1987; Park 1986; Weede 1981, 1986), and (2) many studies used the logged ordinary least squares (LOLS) method for statistical estimation. As will be demonstrated, LOLS is not appropriate when event count data are used for measurements of dependent variables. The unsuitability comes about because event count variables typically display a Poisson or other count-based distribution; therefore LOLS produces inefficient or biased estimates (King 1988, 1989a–b).

Muller and Seligson (1987) recently examined the relationship between inequality and political violence. Using the LOLS method, they concluded that income inequality (the share of income going to the top 20% of all households) has a strong positive effect on mass violence. I shall replicate this study to demonstrate the unsuitability of LOLS when dependent variables are measured by event count data. I shall then reanalyze their data by using an event count analysis—a statistical technique that has not been used in previous research on political violence but has been successfully employed in studies on presidential appointments of Supreme Court justices (King 1987), on the demand of health insurance and the use of health care services (Cameron and Trivedi 1986), and on patents and research and development activity (Hall, Griliches, and Hausman 1986; Hausman, Hall, and Griliches 1984). The replicated analysis reveals that inequality in general—whether in the form of income or land maldistribution—does not have statistically significant effect on collective violent behavior. Instead, a semirepressive regime and intensity of separatism are the main causes of mass violence.

DIRECT REPLICATION

The frequent adoption of LOLS in the study of violence stems from the fact that the ordinary least squares method assumes a linear and additive function. However, event count data display a Poisson or other count-based distribution, and the relations between variables are not linear in functional forms.[4] In such cases, ordinary least squares solutions do not provide proper estimates. A frequently adopted remedy in the literature is to make a natural logarithmic transformation of the dependent variable so that a log-linear function is acquired. All of the studies on political violence I have mentioned adopt this approach. In many instances, LOLS is not suitable for event count data when zero is one of the observed values. This is a frequent occurrence in the study of political violence. A constant c is often added to the dependent variable Y_i before obtaining a logarithmic transformation, so that a zero value can be avoided, namely, $ln(Y_i + c)$.[5] As will be demonstrated, the value of this added constant has substantial effects on the conclusions reached.

Table 1 reports the LOLS results of a direct replication of Muller and Seligson's study, with a small constant, c, taking on different values added to the dependent variable, the logged death rate of 1973–77.[6] Since Monte Carlo experiments have shown that there is no optimal choice for c across empirical examples (King 1988), the values of this constant are arbitrarily set at .001, .01, and 1. As columns 1–3 show, when the value of c changes, the magnitudes of all regression coefficients and their corresponding standard errors also change. The size of almost all LOLS coefficients drops as c increases. For instance,

TABLE 1

LOLS Regression of Political Violence on Income Inequality and Other Explanatory Variables

VARIABLES	C = .001	C = .01	C = 1
Upper 20 % income share	.08	.07	.038*
	(.05)	(.04)	(.017)
ln energy consumption per capita	.46	.30	.11
	(.34)	(.25)	(.11)
Intensity of separatism	2.71*	2.14*	1.23**
	(1.10)	(.82)	(.36)
Semirepressive regime[a]	4.17**	3.06**	.98**[a]
	(.97)	(.73)	(.32)
ln negative sanction per 1m			
1973–77	1.11	.95	.33
	(.69)	(.52)	(.23)
1968–72	−.22	−.06	.13
	(.80)	(.60)	(.26)
Adjusted R^2	.44	.47	.46

Note: Figures in parentheses are standard errors.

[a] The regression coefficient of "Semirepressive regime" is different from that reported in Muller and Seligson 1987, panel 1.3. While all other coefficients are identical to the original ones, it is empirically impossible to have a different coefficient for "Semirepressive regime". I hence conclude that the difference is due to a misprint in Muller and Seligson (1987). N = 61.

*$p < .05$.

**$p < .01$.

the coefficient of semirepressive regime changes from 4.17 to .98—a decrease of 76%—as *c* increases from .001 to 1. In addition, the level of significance also changes. Income inequality (upper 20% income share), for example, is not significant when *c* is .001. As *c* increases to 1, it is significant at the .05 level. The arbitrary choice of an added value for a logarithmic transformation can thus have enormous effects on one's substantive conclusions. This raises additional questions about the accuracy and reliability of previous findings.

INDIRECT REPLICATION: AN EVENT COUNT ANALYSIS

This analysis demonstrates that the commonly adopted LOLS regression analysis is inappropriate when political violence is measured by event count data. The use of event count analysis represents a more reliable and accurate alternative (Cameron and Trivedi 1986; King 1988, 1989a–b). The advantages of event count analysis are (1) it takes into account the nonnegativity of event count data and leaves no possibility of predicting a negative value (least-squares techniques, by contrast, do not constrain the expected number of events to positive values, which must be the case)[7] and (2) since zero is a natural outcome of event count models, there is no need for the logarithmic transformation and hence no "zero problem" (Hausman, Hall, and Griliches 1984).

Probably the simplest specification of an event count analysis is the exponential Poisson regression (EPR) model. Specifically, the EPR model takes the form of

$$E(Y_i) = \lambda_i = \exp(\mathbf{X}\boldsymbol{\beta}) = \exp\left(\sum_{k=1}^{p} X_k\beta_k\right), \qquad (1)$$

where $i = 1 \ldots N$, and $p < N$; $E(Y_i)$ is the expected value of the dependent variable; $\mathbf{X}$ is a vector of independent variables; and $\boldsymbol{\beta}$ is a vector of regression estimates. The goal is to estimate the vector of regression estimates, $\boldsymbol{\beta}$, in which each element represents the individual effect of X_i, a particular independent variable, on the dependent variable or the observed event count.

The Poisson specification is restrictive in one important way, however: it is based upon assumptions that events occur *independently* over time and that the probability of an event occurring at any point of time is *constant*. These assumptions may not be valid, since either the prior occurrence of an event such as political violence or changes of other factors such as economic conditions could result in a change of the probability of subsequent events. In addition, it is common practice in cross-national studies to aggregate data over a period of time (e.g., 5 or 10 years). The aggregation process may increase cross-national variability since countries tend to have different incident-proneness at different time periods. A technique that explicitly takes into account *overdispersion*[8] in event counts is *negative binomial regression*. Statistically speaking, this means that the specification of the variance of Y_i is different in the EPR and negative binomial models even though they have an identical specification of expected values (see equation 1). While the EPR model assumes that the expected value and variance of Y_i are equal (i.e., $V(Y_i) = \lambda_i$), negative binomial models relax this assumption.

Cameron and Trivedi (1986) have shown that two negative binomial models—what they called NEGBIN I and NEGBIN II—can be generated by different specifications of variance:

$$V(Y_i) = (1 + \alpha)\lambda_i \text{ (NEGBIN I)} \qquad (2)$$

$$V(Y_i) = \lambda_i(1 + \alpha\lambda_i) \text{ (NEGBIN II)} \qquad (3)$$

The adequacy of the Poisson model is thus based on tests of the nuisance parameter, $\alpha = 0$. If α is not significantly different from zero, which implies that events are independent, the expected value and variance of Y_i are equal and the negative binomial models will reduce to exactly to the Poisson model. If α is greater than and significantly different from zero, which implies that events are overdispersed and/or have a heterogeneous probability of occurrence, the Poisson model should be rejected. Meanwhile, the choice between NEGBIN I and II is also contingent on tests of α in equation 2 and 3.

With these statistical qualifications in mind, I will replicate Muller and Seligson's study. There are two modifications in the operationalization. First, Muller and Seligson undertook a logarithmic transformation on three independent variables: energy consumption per capita, negative sanctions per one million of 1973–77, and the same of 1968–72. The following analysis does not have that transformation on these variables, since an event count analysis assumes a nonlinear function, so that there is no reason to make a logarithmic transformation on independent variables to fit the linear assumption. Second, the dependent variable of the original model, political violence, was measured by "the *logged* sum of annual deaths from domestic political conflict during 1973–77 *divided by midinterval population*" (Muller and Seligson 1987, 433–34, emphasis mine). No such transformation was done on the dependent variable in the following analysis. Instead, the natural logarithm of the midinterval population (l*n* POP75) is included on the right-hand side as an additional independent variable. This is done because the original form of the relationship is

$$\frac{E(Y_i)}{\text{POP75}} = \exp\left(\mathbf{X}\boldsymbol{\beta}\right)$$

or, equivalently,

$$E(Y_i) = \exp\left(\sum_{k=1}^{p} X_k\beta_k + \ln\left(\text{POP75}\right)\right).$$

This exercise is due to the fact that a linear transformation on the dependent variable (such as taking an average) is incompatible with event count analysis. This method of measurement is conceptually and algebraically identical to the original operationalization. That is, political violence is measured by the death rate from domestic conflict per one million population. Algebraically, this implies that the coefficient of l*n* (POP75) should be constrained to one. It is harmless, however, to free it from this restriction by including it as an independent variable in the equation (King 1988, 1989a).

Furthermore, Muller and Seligson exclude all land inequality variables in the full model, based upon the initial analyses of the relations between all inequality variables and political violence (1987, 435–36). Since their initial analyses are mainly based upon bivariate regression/correlations, the exclusion of land inequality variables from the subsequent analyses is not appropriate. Any relationship or lack of relationship between variables in an incomplete specification such as this may either be spurious or suppressed (Pedhazur 1982). The following analysis will include all inequality variables except "Gini land concentration."[9] As Prosterman pointed out, it is implausible to expect that the relatively high Gini coefficients of land concentration in industrialized societies, where only a small agricultural labor force is present, would generate significant discontent (1976, 350).[10]

Several countries register extremely large scores on the dependent variable. In order to reduce the problem of estimation, Muller and Seligson set a ceiling on the values of the dependent variable. The death rates of these countries were arbitrarily set to 50 (1987, 434).[11] Their decision to set an upper limit to the death rate is inappropriate, because an arbitrary ceiling could skew the conclusions. I shall include the original values of these cases and examine their robust standardized residuals[12] to determine whether their presence would bias the results.[13]

Before the actual effects of the inequality variables on political violence can be examined, it is necessary to determine whether a Poisson model is adequate. As noted, the adequacy is contingent upon tests of the nuisance parameter, α, being significantly greater than zero. Consider the Poisson residuals, $u_i = y_i - \hat{\lambda}_i$, and let $\hat{V}(y_i) = (y_i - \hat{\lambda}_i)^2$. The following regressions can be obtained (standard errors in parentheses are obtained using White's heteroscedastic consistent estimator):

$$\hat{V}(y_i) = \underset{(771.53)}{1{,}458.27}\ \hat{\lambda}_i \tag{4}$$

$$\hat{V}(y_i)/\hat{\lambda}_i = \underset{(324.13)}{530.54} - \underset{(.63)}{.63}\ \hat{\lambda}_i. \tag{5}$$

From equations 2 and 4, it can be seen that $\hat{\alpha}$ = 1,458.27 − 1. The estimated *T*-statistic is 1.89, which is statistically significant at the .05 level with a one-tailed test.[14] This evidence clearly rejects the adequacy of a Poisson specification in favor of a negative binomial parameterization.

Which of the two negative binomial models should be used? The $\hat{\alpha}$ in equation 5 is not statistically significant at the .05 level. This suggests that the first rather than the second negative binomial model is adequate. Table 2, column 1 presents the results with a NEGBIN I parameterization.[15]

Surprisingly enough, none of the regression coefficients in Table 2, column 1 is statistically significant at the .05 level for a two-tailed test. Only intensity of separatism and semirepressive regime are of borderline significance (between the .05 and .10 levels). An analysis of the residuals reveals that Argentina, Peru, and the United Kingdom are outliers.[16] In order not to be biased by a few outliers, I drop these three cases and reestimate the parameters. Parallel regression equations to equations 4 and 5 are first obtained to determine an appropriate specification:

$$\hat{V}(y_i) = \underset{(32.83)}{73.01}\ \hat{\lambda}_i \tag{6}$$

$$\hat{V}(y_i)/\hat{\lambda}_i = \underset{(39.88)}{99.53} + \underset{(.05)}{.02}\ \hat{\lambda}_i. \tag{7}$$

Equation 6 again suggests that the negative binomial is preferred to the Poisson specification; with $\hat{\alpha}$ in equation 7 being statistically insignificant, it suggests that the NEGBIN I is still a preferred specification.

Table 2, column 2 presents the results after the outliers are removed from the analysis. Again, as in column 1, none of the inequality variables has statis-

TABLE 2

Negative Binomial Regression of Political Violence on Inequality and Other Explanatory Variables

VARIABLES	ALL VALID CASES INCLUDED (N = 45)	OUTLIERS EXCLUDED (N = 42)
Agrarian inequality	−.016 (.023)	−.043 (.031)
Landless	−.004 (.027)	.048 (.032)
Upper 20% income share	.032 (.029)	.032 (.026)
Energy consumption	−.0001 (.012)	.002 (.012)
Intensity of separatism	.733 (.408)	1.180** (.434)
Semirepressive regime	.984 (.508)	1.369** (.550)
Negative sanctions per 1m		
1973–77	−.001 (.032)	.012 (.034)
1968–72	.068 (.116)	−.005 (.108)
ln (Population, 1975)	.297 (.166)	.171 (.150)
Log-likelihood	82,224.5	43,258.7

Note: Figures in parentheses are standard errors.
$^{**}p < .01$.

tically significant effect on collective violence. This finding contradicts Muller and Seligson's strong claim that income inequality is the major determinant of mass violence. It also runs counter to the argument that landlessness, the proportion of the labor force that does not own or possess ownershiplike tenure of land, contributes to violence (Prosterman 1976; Prosterman and Riedinger 1987). Agrarian inequality (the multiplicative interaction between land inequality and the size of the agricultural labor force) also fails to warrant a causal effect on collective violent behavior. The evidence presented here indicates that inequality in general does not have statistically significant effect on mass violence.

The only variables that are associated with political violence are semirepressive regime and intensity of separatism, which serves as a control variable. The positive effect of intensity of separatism can be explained on the grounds that political violence often occurs when groups or regions attempt to gain greater autonomy or independence. The significant semirepressive regime lends some support for the resource mobilization interpretation of political violence. Its advocates argue that the determinant of collective violence is not the discontents generated from inequality per se but rather the organization of discontent.[17] Individuals are more likely to be organized and engage in violent behavior in a semirepressive regime than in a repressive one. Differences in regime structure are thus relevant because they directly affect individuals' ability and likelihood to get organized.

Moreover, semirepressive regimes also serve as causes of collective violent behavior. The Iranian revolution documented in detail by Green (1982, 1984) and cited by Muller and Seligson (1987, 430) is a case in point. Green argues that the pseudoparticipation tactics undertaken by the shah "served to increase popular hostility among those socially mobilized Iranians eager to have a measure of influence over the manner in which their society was ruled" (1984, 155). This unrealized desire for real power in a semirepressive regime can thus become a contributor to collective violence. As a result, a semirepressive regime is not only a facilitating factor but also a direct cause of political violence.

CONCLUSION

My analysis indicates that Muller and Seligson's previous findings are suspect due to the inappropriate statistical methods employed. The new evidence suggests that inequality in the forms of income or land maldistribution does not have effects on political violence. Rather, regime structures and intensity of separatism are more important in explaining the occurrence of political violence. Semirepressive regimes are relevant not only because they allow individuals to get organized and engage in violent behavior but also because they are causes of violence.

The fact that none of the inequality variables is statistically significant does not mean that the relative deprivation thesis is theoretically meaningless. In Gurr's (1970) conceptualization of relative deprivation, discontent is not generated from inequality per se, but rather from the gap between an individual's expected and achieved well-being. Indices based on measures of objective inequality cannot, by themselves, purport to assess this crucial part of Gurr's hypothesis. Inequality in the form of income or land maldistribution will not be translated into widespread discontent if there is no perceived discrepancy between what people actually get and what they expect to get. It is necessary, therefore, to assess individuals' perception of this gap through survey data before the relative deprivation hypothesis can be tested—an argument made more than a decade ago (Eckstein 1980; Weede 1981).

The methodological implication of this analysis, however, goes well beyond the study of political violence. There are many other studies in the discipline that use event count data as the measurements of dependent variables. The number of coups, coup attempts, presidential vetoes, and presidential appointments of Supreme Court justices are only a few examples. Far from being only a problem of technical differences, the widely used conventional method can lead to incorrect or misleading substantive findings, as demonstrated in the reanalysis of Muller and Seligson's data. Least squares solutions provide inefficient and biased estimates when the dependent variable uses event count data. An event count analysis is the appropriate technique for statistical estimation, since the underlying data generation process of the dependent variable is a Poisson or other count-based distribution.

T. Y. Wang

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RESPONSE

T. Y. Wang attacks cross-national studies of political violence on grounds that they routinely rely on faulty estimation methods. Similarly, Brockett has claimed that the conventional dependent variable in such studies, deaths from political violence, is "mortally flawed" by measurement error (1992, 169). In regard to estimation methods, Wang directs two key criticisms at contemporary conflict studies, which, if valid, undermine the empirical foundation for the proposition that income inequality promotes political violence. He asserts (1) that the common practice of logging dependent variables derived from counts leads to capricious and unreliable results and (2) that when estimated by appropriate maximum likelihood techniques, the relationship between inequality and political violence disappears. Based on documentary evidence for Central American countries, Brockett demonstrates that counts of deaths from political conflict events from the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983) are grossly underreported for several countries. He infers that this kind of measurement error is likely to be prevalent for the Third World in general and, if so, that the *World Handbook* data are too inaccurate to permit meaningful cross-national testing of hypotheses about determinants of political violence.

These criticisms are more than methodological quibbles about the validity of the finding of a positive effect of income inequality on political violence. They seriously challenge the empirical foundation underpinning our substantive understanding of the macrosocietal causes of political violence in general. We shall address each criticism. As we shall show, these criticisms fail to overturn previous results.

LOGARITHMIC TRANSFORMATION OF CONFLICT MEASURES

Wang's initial criticism is leveled at the near universal practice of transforming the dependent variable into logarithms prior to estimation by ordinary least squares (LOLS). He argues two separate briefs against this procedure. The first hinges on the observation that violent deaths, death rates, and similar count-based conflict measures commonly used as dependent variables cannot possibly assume negative values. Least squares techniques are therefore conceptually inappropriate because they can provide no assurance that predicted values will be positive. The second argument arises from the fact that zero is a typical value of these dependent variables and since the logarithm of zero is undefined, some small arbitrary constant must be added to these measures. He presents results showing that the choice of a constant entails serious consequences for subsequent estimates and for substantive conclusions drawn from them. We address each of these issues in turn.

The objection to negative predicted values with count-based measures is a special case of a more general conceptual problem with ordinary least squares that can occur whenever a dependent variable is theoretically constrained to a particular range or set of values. The problem is most often discussed in the context of dichotomous dependent variables, though it obviously applies with equal force to event counts. We certainly have no quarrel with Wang's position here. We, too, prefer models that impose some constraint on predictions of negative deaths or death rates. However, we do quarrel with Wang's implication that LOLS models are just as susceptible to this problem as standard ordinary least squares. In fact, LOLS always imposes some level of constraint on negative predictions.

It is important to keep in mind that the dependent variable of an LOLS model is expressed in logarithms and therefore its predicted values are also in logarithms. It is probably fairly common for LOLS estimates on counts to produce negative predictions but these values are not themselves counts, they are *logged* counts.[18] Since negative logarithms map within the range of zero to one, the problem of negative counts or rates is averted. One might still object to a prediction of, say, half a death; but fractions routinely occur elsewhere as well and are not really at issue.

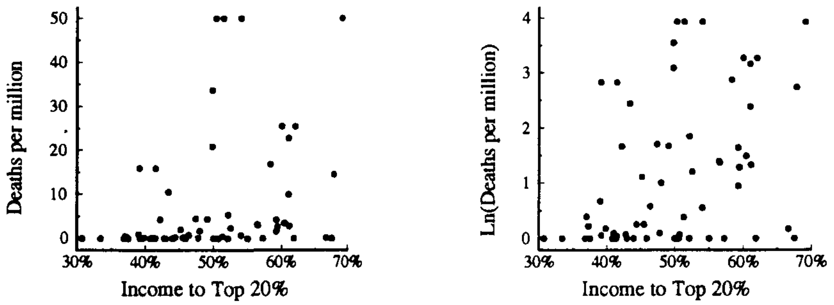
It is true that under some conditions the LOLS constraint on negative predictions may be only a bounded constraint. This will occur whenever a positive constant is added to an original measurement so that logarithms of zero counts can be taken. In this case, LOLS can produce negative predictions, but only within a fixed range determined by the magnitude of the constant. For example, suppose an LOLS model on raw death counts produces a predicted value of $-.69$ which, after exponentiation, is equivalent to one-half a death. If the original data had been incremented by a constant larger than this predicted count, then removal of the constant would yield a negative prediction. Notice that a negative result can only occur from subtraction of a constant and that the range of negativity is strictly bounded by the constant's size. In our view this is nothing more than a minor nuisance.

The addition of a constant prior to taking logarithms is at the heart of Wang's second line of argument against LOLS estimation. The purpose of

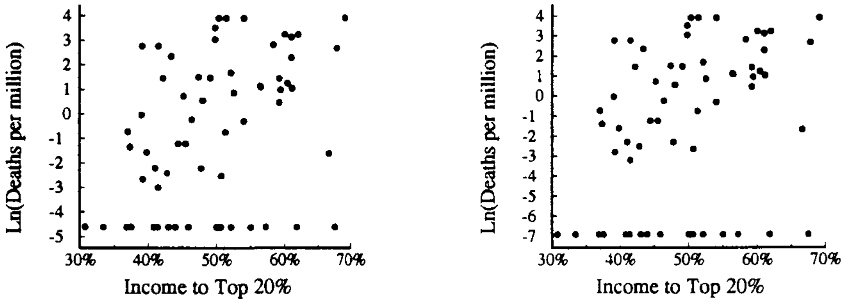
FIGURE 1

Inequality and Deaths per Million Logged with Different Increments

A. Raw Deaths per Million **B. Ln(Deaths per Million + 1)**



C. Ln(Deaths per Million + .01) **D. Ln(Deaths per Million + .001)**



Note: Deaths per million are totals for 1972–77 divided by 1975 population. Inequality Data is ca. 1970. There are 62 valid observations.

this operation is to transform zero counts or rates into positive values, and since any positive constant will do the job, Wang maintains that the choice of constant is essentially arbitrary. This is a critical point because he goes on to show in his Table 1 that the choice of constant affects the magnitudes of subsequent parameter estimates and their standard errors in such a way that substantive conclusions are called into question.[19] Most importantly, with the addition of .001 instead of 1.0—the value conventionally used in conflict studies[20]—the estimated effect of income inequality on political violence fails to be significant at the .05 level even by a one-tailed test. He concludes that one can have little confidence in substantive conclusions based on LOLS estimates that are so drastically influenced by an arbitrary constant.

Wang is obviously correct in claiming that any positive constant added to zero will permit transformation to logarithms. In this sense, the choice of constant is arbitrary. We take issue, however, with his unstated assumption that this choice is also free of any substantive implications: it is not. In particular, we shall argue that under certain circumstances a constant can introduce step-level scaling changes that may or may not correspond with prior theoretical assumptions. Thus, while the need for a constant may be purely methodological, the particular value chosen must be consistent with prior theory.

We can best illustrate these theoretical implications by focusing on the relationship Wang finds most seriously affected by the choice of constant—that between income inequality and death rates (per million population) from political violence. Figure 1 displays four scatterplots for the 62 countries with valid observations on both measures.[21] Plot A in the upper left shows the relationship between inequality and raw death rates. Plots B, C, and D use logarithms of death rates with increments of 1.0, .01, and .001, respectively. In the unlogged case (plot A) the relationship is evidently exponential. Specifically, the death rate from political violence is a positively accelerated function of income inequality. This is consistent with the hypothesis that structural conditions of discontent must reach relatively high levels for mobilization in the form of rebellious collective action to occur (Muller 1985, 53).

When deaths per million are logged with a constant of 1.0 in plot B, the transformed relationship approaches linearity with minimal changes to the scaling at low-rate levels, which is consistent with the hypothesis of a positively accelerated function. Plot C with a constant of .01, however, is quite different.

Here we observe a gap beginning to open between observations having the lowest observed nonzero death rates and those with zero deaths. This gap becomes even more pronounced with an increment of .001 in plot D. The plots in Figure 1 demonstrate, with some clarity, why Wang's LOLS results produced parameter estimates for income inequality that became smaller in relation to their standard errors as the constant adjustment became smaller. As the interval between zero and very small death rates widens, observations are moved away from any least squares line and standard errors become progressively inflated.

The distance between zero and nonzero rates or counts respond in this way because the logarithmic function is symmetrical around one. Thus, as fractions between zero and one become smaller, the magnitude of their logarithms (expressed in negative numbers) increases at an accelerating pace, paralleling the acceleration of positive values above unity. This has an obvious but strangely counterintuitive implication for the choice of a constant value for taking logarithms. Intuition might suggest a preference for .001, rather than 1.0, on grounds that the smaller the value added to a measure the less distortion it will introduce. But in fact, as fractional constants become smaller their logarithms become progressively larger negative numbers.

A table of logs (or, to be more contemporary, a moment with a hand calculator) will immediately reveal how a very small fractional constant like .001 added to an integer greatly expands the distance between zero and one in comparison to the nearby intervals between one and two, two and three, and so on. Moreover, by the symmetry of the logarithmic function, a constant adjustment of .001 necessarily represents the logged interval between zero and one as equivalent to the logged distance between one and one thousand. Substantively, this implies that a single death from political violence constitutes a huge step-level change from zero deaths. This step-level representation does not conform to our own hypothesis of a smoothly accelerating function, nor does it conform to any hypotheses that have been expressed by other conflict theorists. Indeed, we cannot conceive of any meaningful theoretical justification for such a representation.[22]

The problems caused by small fractional constants are even more severe with rates. At least with counts the distortion is confined to a single interval. But rates less than one are affected even more perversely since distances between logged rates grow progressively larger as the observed rates themselves become smaller. Frankly, we cannot begin to imagine what sort of conflict theory could justify such a result. It strikes us as pure nonsense.

In contrast, addition of a constant 1.0 before taking logarithms progressively compresses all observed counts and rates as they become larger, both within and beyond the zero–one interval. This is a theoretically appealing result for conflict studies because it appropriately linearizes the hypothesis that political violence is a smoothly accelerated positive function of income inequality. Of course, fractional constants yield the same linearizing result for observed counts and rates greater than one. Because fractional constants carry only a small and diminishing effect for values greater than one, it is only in the zero–one range that serious problems arise, which is possibly why the implications discussed here were overlooked by Wang.

Thus for reasons having to do with both our theoretical assumptions about political violence and the symmetry of the logarithmic function, a constant adjustment of 1.0 is anything but arbitrary.[23] In fact, the addition of 1.0 to counts and rates is the only way to preserve what we regard as theoretically meaningful distances in the zero–one range. Furthermore, in our view the theoretical utility associated with a constant of 1.0 substantially outweighs the minor nuisance of bounded negative predictions noted earlier. Accordingly, we do not regard Wang's criticism of LOLS estimation to be sufficiently sound to undermine previous findings of the relationship between inequality and political violence. Yet such a relationship would at least come under suspicion if it failed to stand up under alternative estimation methods. Wang claims he has shown this to be the case. It is to this question that we now turn.

EXPONENTIAL POISSON REGRESSION

Wang's second main criticism of conflict studies is equally severe. Because he regards LOLS estimates with an added constant as capricious and unreliable, he argues for an alternative estimation method based on the EPR model. He then presents maximum likelihood EPR estimations on raw death counts that diverge sharply from previously published LOLS results using logged death rates. His most important finding—that inequality has no impact whatsoever on political violence—has consequences beyond our study (Muller and Seligson 1987), for it appears to undermine a conclusion of several other cross-national analyses as well. Or does it?

The EPR analysis in Wang's Table 2 is termed an "indirect replication" of ours (Muller and Seligson 1987) because it differs on numerous operational and design issues. The most important of these differences is Wang's decision to base his conclusions entirely on a single specification, including two land distribution measures along with income inequality. We believe that this is an inappropriate decision for several reasons. In the first place, the distribution of land and income are not of equal theoretical status. For reasons specified elsewhere, income inequality is theoretically a more proximate cause of political violence than land distribution; therefore, it is the former that merits closest scrutiny (Boswell and Dixon 1990; Muller and Seligson 1987). Wang is right, however, in pointing out that an observed relationship between income inequality and violence could still be the spurious result of land maldistribution affecting both.

TABLE 3

Regression of Political Violence on Income Inequality and Other Explanatory Variables

VARIABLE	MODEL 1 (N = 62) NEGBIN	MODEL 1 (N = 62) LOLS	MODEL 2 (N = 61) NEGBIN	MODEL 2 (N = 61) LOLS
Upper 20% income share, ca. 1970	.030* (.014)	.042* (.017)	.036* (.015)	.051* (.019)
Intensity of separatism, 1975	.959* (.230)	1.454* (.393)	1.054* (.389)	1.525* (.397)
Semirepressive regime, 1973–77	.767* (.237)	.874* (.278)	1.087* (.465)	1.136* (.355)
Negative sanctions per 1m, 1975[a]	.018 (.012)	.475* (.218)	.015 (.016)	.361 (.227)
ln (Population, 1975)	.261* (.069)	—	.213* (.109)	—
Energy consumption per capita, 1970[a]	—	—	.011 (.007)	.188 (.125)
Constant	.752* (.398)	−1.906* (.826)	.490 (.628)	−3.627* (1.501)
Dispersion parameter $\hat{\sigma}^2$	1,011.3*	—	979.5*	—
Log-likelihood	115,029	—	115,033	—
Adjusted R2		.45		.46

Note: Main entries are negative binomial regression estimates on raw death counts (NEGBIN) or LOLS estimates on death rates plus 1; entries in parenthesis are standard errors.

[a] Variable is logged in LOLS estimation.

*$p \leq .05$, one-tailed test.

Furthermore, while Wang's specification is not without some substantive justification, its inclusion of manifestly irrelevant variables pays a high price in lost efficiency and degrees of freedom. The latter problem is particularly severe in this context since the pattern of missing observations across the income and land measures has the deleterious consequence of reducing the sample size by approximately 25% from what would be available to models employing income inequality alone. Inexplicably, Wang elected not to follow the standard practice of "testing down" or "trimming"—that is, of reestimating his model with the irrelevant variables excluded. We shall show that had he done so, he would have arrived at different conclusions.

Thus both theoretical and methodological considerations lead us to suspect that Wang's EPR estimates may be more an artifact of his specification than results with revolutionary substantive implications as he claims. We therefore set out to replicate Wang's "replication" with a few improvements of our own. Our analyses follow the conventional procedure of beginning with the most causally proximate and theoretically parsimonious specification prior to testing for spurious effects. We omit considerations of negative sanctions for 1968–72 because it has failed to produce significant results in any of our LOLS or EPR estimations (Muller and Seligson 1987, 437). We also depart from Wang's reliance on two-tailed significance tests in favor of the one-tailed tests we originally used (though this is a minor point that turns out to have no bearing on our conclusions). Finally, because the method of estimation is a point at issue, we provide LOLS estimates alongside EPR results to permit valid comparisons across equivalent specifications.

Our dependent variable for EPR estimation is cumulative deaths from political violence during 1973–77, controlled for population size by the log of 1975 population on the right-hand side. We follow Wang's use of negative binomial regression, since we would expect such deaths to be correlated with one another—an expectation that implies a positive and significant estimate for the dispersion parameter.[24] For LOLS estimation, we used cumulative deaths per million 1975 population as the dependent variable and dropped population from the right-hand side.[25] Death rates were transformed to logarithms after receiving a constant increment of one. Two other variables on the right-hand side—negative sanctions per million and (in model 2) energy consumption per capita—were also transformed to logarithms for LOLS estimation.

Results from our estimations are presented in Table 3. The initial specification labeled model 1 not only reflects our stated theoretical emphasis on income inequality but also affords the broadest possible cross-national comparison over a sample of 62 countries.[26] Examination of negative binomial estimates on the left and LOLS on the right reveals a high degree of convergence in the signs and robustness of estimated effects. Of particular interest is the fact that both estimation methods produce positively signed estimates for the income inequality measure. Because

TABLE 4

Regression of Political Violence on Income Inequality, Land Inequality, and Other Explanatory Variables

VARIABLE	MODEL 3 (N = 83)		MODEL 4 (N = 63)		MODEL 5 (N = 46)	
	NEGBIN	LOLS	NEGBIN	LOLS	NEGBIN	LOLS
Agrarian inequality	.002	.009	—	—	−.019	−.017
	(.008)	(.009)			(.001)	(.016)
Landlessness	—	—	.011	.011	.001	−.006
			(.008)	(.016)	(.374)	(.021)
Upper 20% income share, ca. 1970	—	—	—	—	.040*	.064*
					(.014)	(.030)
Intensity of separatism, 1975	.704*	1.720*	.723*	1.161*	.816*	1.264*
	(.152)	(.379)	(.046)	(.347)	(.005)	(.442)
Semirepressive regime, 1973–77	.462*	.538*	.521*	.516	.896*	.887*
	(.308)	(.323)	(.043)	(.344)	(.005)	(.392)
Negative sanctions per 1m, 1975[a]	.025*	.674*	.018	.475*	.013	.329
	(.015)	(.195)	(.380)	(.202)	(.260)	(.228)
ln (Population, 1975)	.269*		.266*		.256*	
	(.027)		(.113)		(.005)	
Constant	3.638*	−.380	2.057*	.112	1.117*	−2.149*
	(.203)	(.470)	(.051)	(.291)	(.005)	(1.094)
Dispersion parameter ($\hat{\sigma}^2$)	6,003.9*	—	739.8*	—	897.1*	—
Log-likelihood	694,192	—	87,700	—	82,221	—
Adjusted R^2	—	.35	—	.26	—	.35

Note: Main entries are negative binomial regression estimates on raw death counts (NEGBIN) or LOLS estimates on death rates plus 1; entries in parenthesis are standard errors.

[a] Variable is logged in LOLS estimation.

*$p \leq .05$, one-tailed test.

we regard the sign of the coefficient as vital to its interpretation we employ a one-tailed test, yet these inequality estimates would remain significant even by more stringent two-tailed standards. Although both estimates are substantively interpretable as nonlinear effects, they are expressed for differently scaled dependent variables and cannot be directly translated to a fully comparable common metric.[27]

Results from our model 1 specification show that income inequality does contribute to political violence and, contrary to Wang, that this conclusion is unaffected by the method of estimation. However, these estimates alone are not sufficient to identify the source of the discrepancy between Wang's results and our own, nor do they address the possible causal role of land maldistribution. We explore these issues with some additional estimations beginning with model 2 in Table 3. First, we consider Wang's inclusion of energy consumption per capita as a control for economic development. Note that this specification forfeits two degrees of freedom, since the introduction of energy consumption results in the loss of one observation. Results listed in the two columns for model 2 reveal that model 1 estimates are little affected by the inclusion of energy consumption. By most accounts, economic development should be negatively related to political violence; yet both methods reveal positively signed coefficients, though neither reaches significance even for a two-tailed test.[28] Furthermore, Wang's own results show that the effect of energy consumption is virtually nil. Accordingly, energy consumption can be safely ignored in further analyses.

We have yet to examine the role of land distribution. We undertake this task in Table 4 with specifications that include each land measure individually and a specification similar to Wang's that encompasses both land variables and inequality. Model 3 is identical to model 1 except that it substitutes agrarian inequality for income inequality. Agrarian inequality is a composite indicator consisting of the Gini index of land concentration weighted by the size of the agricultural labor force.[29] Our estimations are conducted over the broadest possible sample consisting of all 83 countries for which this measure is available. It is evident from columns 1–2 that neither estimation method is able to detect any discernable effect of agrarian inequality on political violence.

Model 4 follows the same procedures, substituting landlessness for agrarian inequality. Landlessness is measured by agricultural households without land as a proportion of the total labor force. While this is not an ideal indicator of land-based discontent, it is nevertheless preferable to measures based on the Gini index (Brockett 1992; Prosterman and Riedinger 1987). Even so, estimations over all 63 countries for which data are available once again reveal no effects attributable to the maldistribution of land. And once again, the two estimation methods yield comparable results.

We believe that the evidence adduced so far would lead most analysts to conclude that income inequality does promote political violence but that agrarian inequality and landlessness do not. Nevertheless, we examine one final specification that includes all three variables. Because the pattern of missing data across the three measures leaves a sample of only 46 countries, our basis for comparison is unavoidably diminished. Both sets of estimates for model 5 displayed in Table 4 columns 5–6 again show that income inequality contributes to political violence and that agrarian inequality and landlessness are immaterial. This is an especially interesting result for two reasons. First, even though the specification differs only marginally from Wang's by excluding two manifestly irrelevant variables (energy consumption and lagged negative sanctions), the resulting EPR estimates lead to strikingly different conclusions. And second, this estimation corroborates the pattern of results found for the individual estimations in models 1, 3, and 4. Certainly there is no evidence from any of our estimations to support the continued inclusion of land distribution measures. And excluding them takes us back to model 1, which we hypothesized as the proper specification in the first place.

To this point we have focused our attention on Wang's most provocative claim, namely, that EPR estimation overturns the finding of previously published studies showing income inequality to impart a positively accelerated effect on deaths from political violence. We have demonstrated that about the only evidence for this claim is to be found in Wang's lone estimation of what is apparently an idiosyncratic specification encompassing numerous irrelevant variables over a particular subset of cases. Indeed, the weight of present evidence, irrespective of estimation method, clearly supports the original conclusion that income inequality does promote violence. But to say that the two estimation methods concur in the identification of *some* positive inequality effect is not necessarily to say that they identify the *same* effect. Despite the coincidental similarity of the EPR and LOLS estimates displayed in our tables, the size of the effects are not directly comparable since they apply to differently scaled dependent variables under differently conditioned specifications. In order to undertake such a comparison we must depart from the issue as originally posed by Wang to consider directly parallel specifications.

An appropriately parallel specification can be constructed from the elements of our model 1 estimations in Table 3, though this new formulation is not exactly identical to either specification in its original form. Comparability of dependent variables is achieved by redirecting the LOLS specification to an estimation on raw death counts (logged with an increment of 1.0) and adding the logarithm of population to the righthand side.[30] With this modification the only remaining difference is in the representation of negative sanctions, which is logged in LOLS and unlogged in EPR. Until now we have followed Wang's procedure of using unlogged sanctions for EPR estimation. Here, however, we depart from Wang by following the more common practice of logging sanctions in both specifications (e.g., Hibbs 1973; Muller and Seligson 1987).

TABLE 5

Regression of Politically Violent Death Counts on Income Inequality and Other Explanatory Variables (N = 62)

VARIABLE	NEGBIN	LOLS
Upper 20% income share, ca. 1970	.037*	.070*
	(.012)	(.026)
Intensity of separatism, 1975	.892*	2.004*
	(.236)	(.593)
Semirepressive regime, 1973–77	.644*	1.391*
	(.225)	(.484)
ln (Negative sanctions per 1m, 1975)	.360*	.758*
	(.126)	(.322)
ln (Population, 1975)	.332*	.685*
	(.075)	(.160)
Constant	−.474	−8.684*
	(.394)	(2.201)
Dispersion parameter ($\hat{\sigma}^2$)	964.7*	—
Log-likelihood	115,031	—
Adjusted R^2		.59

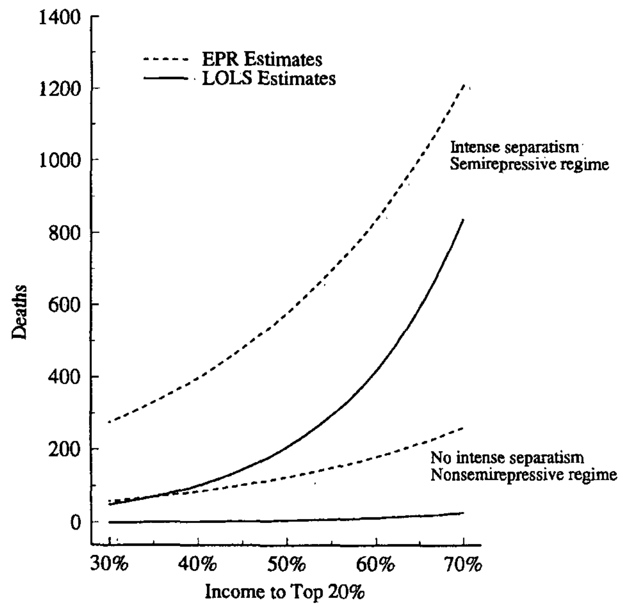
Note: Main entries are negative binomial regression estimates on raw death counts. (NEGBIN) or LOLS estimates on death counts plus 1, entries in parenthesis are standard errors.
*$p \leq .05$, one-tailed test.

Empirical estimates for this new parallel specification are listed in the two columns of Table 5. There are, of course, both similarities and differences depending on one's point of view. Lacking both fully specified theories and precise measurements over complete sets of observations, contemporary cross-national social scientists seldom hypothesize exact magnitudes of effects. Ordinarily, about all we can ask of our nascent theories and meager data is to identify that there is some effect in the predicted direction within a specified standard of certainty. Sometimes, as in the case of inequality and violence, we can also stipulate an expected functional form. From this conventional perspective the two sets of estimates in Table 5 bear a remarkable similarity. Both methods reveal a positively-accelerated inequality effect on deaths while holding population constant; both identify the important role of separatism and regime repressiveness; and both now show that contemporaneous negative sanctions carry a positive effect under a logarithmic specification.

Beneath the similarities highlighted by conventional social scientific practice are some important differences as well. Most notably, it is apparent that the LOLS estimates achieve systematically larger magnitudes than their EPR counterparts by a ratio of

FIGURE 2

Estimated Inequality Effects on Raw Deaths



roughly two-to-one (except for the constant term, which is larger still). The substantive implications of this size differential are not readily apparent from the reported values themselves since both methods summarize nonlinear functional forms. We can gain at least some limited perspective on these differences by graphing both estimated relationships between income inequality and deaths while holding other variables in the model at some constant value. The mean value is the usual choice of constant for continuous measures and is the value we use for negative sanctions and population. However, the mean is neither informative nor realistic for dichotomous attributes like intense separatism and regime repressiveness, which by our procedures must be either present or absent. Accordingly, we depict each estimated inequality relationship twice, first under conditions that are least likely to produce violence (that is, where intense separatism and semirepressive regime are both absent), and again under the most violent prone conditions (that is, where both are present).[31]

The four graphical depictions are displayed together in Figure 2 with estimated EPR functions represented by broken lines and LOLS functions by solid lines. Two observations seem particularly noteworthy. First, it is evident that EPR and LOLS estimations expose approximately the same functional form under each set of conditions. To be sure, they are not identical—LOLS accelerates somewhat more slowly than EPR under less violent prone conditions and somewhat more rapidly than EPR when violence is more likely. Nevertheless, within the limits of our data they do seem reasonably similar. Second, it is equally evident that LOLS produces systematically smaller estimates of predicted deaths than EPR.[32] The size of this discrepancy varies depending on the level of inequality and the conditions established by separatism and semirepressiveness (though it can be quite large, reaching a high of 420). Clearly this is a difference—the first we have encountered in any of our comparisons—that carries important substantive implications. Given the fact that EPR is specifically tailored to estimations on raw counts, it appears likely that LOLS is underestimating predicted deaths even while delivering a reasonably faithful portrait of the underlying theoretical process.

The results of our analyses have several important substantive and methodological implications for cross-national studies of internal conflict. First and foremost—and contrary to Wang—is the reaffirmation of income inequality as a condition promoting political violence. In addition, our analyses have shown that this inequality effect is not the misspecified result of excluded land maldistribution measures. Indeed, on the evidence at hand, these variables warrant no valid claim to systematic causal relevance. These results are especially impressive because they emerge from quite different estimation methods on different dependent variables. Furthermore, EPR estimation provides some external validation for certain design decisions commonly taken in LOLS estimation. In particular, the EPR results demonstrate that adding a constant 1.0 prior to taking logarithms does not in any way "skew the conclusions," as Wang would have us believe.

More generally, we found a high degree of convergence in the results from our EPR and LOLS estimations across a variety of specifications. Most encouraging is the fact that each set of results taken alone points to exactly the same conclusions regarding the sources of political violence. We must not generalize too far: the two estimation methods do differ in numerous respects and there is no guarantee that they will deliver such similar results in other contexts. Moreover, when examined over comparable specifications on raw counts, we found substantial discrepancies in the predicted values produced by the two techniques. One advantage often attributed to EPR methods is their less-restrictive assumptions. However, for studies of political violence there is at least one respect in which estimation on raw counts may be somewhat more restrictive in practical applications than estimations on rates. We refer to problems of data collection that may impair accurate accounting of deaths and other violent events.

THE ACCURACY OF POLITICAL VIOLENCE MEASURES

We follow the now-standard practice of measuring political violence based on the number of deaths associated with riots, armed attacks, assassinations, and other politically motivated conflict events. Deaths are preferred to other indicators for a variety of reasons itemized elsewhere (Muller and Seligson 1987, 448). Like others, we rely on the death reports compiled in the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). Recently, however, *World Handbook* records have been criticized by Brockett (1992) as inaccurate due to underreporting of deaths. Specifically, he provides documentary evidence of significant underreporting for three nations in Central America: Guatemala, El Salvador, and Nicaragua. Although blanket charges of inaccuracy without supporting evidence can do little to advance inquiry, we believe that closer inspection of data such as these by country and area specialists is a constructive development. We are therefore grateful for Brockett's efforts and hope that his careful scrutiny of Central America is followed by others just as familiar with other regions of the world.

We concur with Gurr's (1974) observation that minor inaccuracies in political violence data are unlikely to have much affect on the general inferences drawn from cross-national studies. We also concur that "gross and systematic" errors are another matter entirely. All the more troublesome is the fact that errors are most likely to be gross and systematic during periods of large-scale insurgency, revolt, and civil war. Our approach to this problem is a bit more sanguine than Brockett's, though we share his intolerance for faulty data. We believe that knowledge of serious underreporting is better used as a remedy than an indictment. One such remedy would have us eliminate from future analyses any cases plagued by documented irregularities in the reporting of political violence. But to do so would surely introduce equally serious distortions, since these are cases where political violence not only is known to occur but does so at relatively extreme levels. An alternative—indeed, in our view optimal—solution would be to correct inaccurate data points with new and more reliable measurements whenever they become available. But there are drawbacks here, as well, for documented evidence of underreporting does not necessarily imply a precise accounting. Brockett's investigation of deaths in Central America is a case in point. While he does supply convincing evidence of underreporting of deaths, he fails to provide enough detailed information to permit an accurate correction.

There is a third course that will allow us to take account of Brockett's information but not require an exact accounting of cumulative deaths. This is made possible by establishment of a censoring point at some value representing moderately but not excessively high levels of violence. In our previous work we have used 50 deaths per million for this purpose. Because we can reasonably infer from Brockett that the underreported cases exceed this threshold, we can adjust our indicator for these cases by setting it to the maximum level of 50 per million.[33] This will permit us to investigate in at least a provisional way how these particular underreported cases may have affected our earlier estimates. Again we must emphasize that this is not our preferred solution, but it does at least permit us a more informed judgment than would otherwise be possible at the present time. Note, too, that because our censoring threshold is expressed as a rate, the adjusted indicator requires estimation by a procedure like LOLS. This is not to say that similar solutions could not be implemented for estimating on raw counts, only that in our view the task becomes a much more formidable one.

For this new estimation we employ a slightly larger sample than before, made possible by the inclusion of four additional countries for which we now have reliable measures of income inequality.[34] Estimates using these threshold-adjusted death rates are presented in Table 6. Column 1 (model 1A) uses the specification of model 1 in Table 4 while the second (model 2A) includes energy consumption per capita and is comparable to model 2. Although the larger sample and death rate adjustments do bring some slight changes to the estimates, along with a marginal decline in overall fit, the general pattern of results remains very close to those in Table 4. We also present a new estimation (model 4) that includes death rates from 1968–72 as a control, thus transforming this specification into a change model corresponding to our original estimation 1.3 (Muller and Seligson 1987). Change specifications of political vio-

TABLE 6

Regression of Corrected Political Violence on Inequality and Other Explanatory Variables

VARIABLE	MODEL 1A	MODEL 2A	MODEL 6
Upper 20% income share, ca. 1970	.030*	.032*	.042*
	(.016)	(.018)	(.016)
ln (Energy consumption per capita, 1970)	—	.102	.239
		(.123)	(.109)
Intensity of separatism, 1975	1.045*	1.063*	.618*
	(.363)	(.367)	(.332)
Semirepressive regime, 1973–77	1.027*	1.218*	.927*
	(.298)	(.340)	(.301)
ln (Negative sanctions per 1m, 1975)	.424*	.348	.357*
	(.210)	(.220)	(.190)
ln (Deaths from pol. viol. per 1m, 1968–72)	—	—	.517*
			(.113)
Constant	−1.314*	−2.128*	−3.876*
	(.787)	(1.450)	(1.310)
Adjusted R^2	.40	.40	.59
N	66	65	65

Note: Main entries are LOLS estimates on death rates corrected with information from Brockett, 1992; entries in parenthesis are standard errors.
*$p \leq .05$, one-tailed test.

lence have the advantage of embedding models in historical context while providing even more stringent controls than the level specifications examined so far.

Our arguments are summarized by three main conclusions. First, logarithmic transformations on count variables are not arbitrary when accompanied by a proper theoretical understanding of the process being modeled. Second, the method of estimation makes virtually no difference in the general conclusions derived from properly specified model. Finally, making allowances for underreporting of deaths by using the best information available does not significantly alter the results reported in previous studies. As a consequence of this exercise, we are now even more confident that high levels of income inequality promote high levels of political violence.

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Notes

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1. For a review of this argument, see also Snyder 1978.

2. For a review of this literature, see Lichbach 1989.

3. "Event counts are variables that have for observation i ($i = 1, \ldots N$) the number of occurrences of an event in a fixed domain" (King 1988, 838). Examples include the numbers of coups, coup attempts, deaths from violence, and so on.

4. For a discussion of these distributions, see Cameron and Trivedi 1986; Hausman, Hall, and Griliches 1984; King 1989b.

5. The most frequently used value for c is 1. For instance, Muller and Seligson indicated that "an increment of one is added to each death score before logging because the log of zero is undefined" (1987, 448, n. 8). Many other studies also added one to the dependent variable before taking a logarithmic transformation (e.g., Boswell and Dixon 1990; Hartman and Hsiao 1988; Muller 1985, 1988b; Park 1986; Weede 1981, 1986).

6. The variables included in this analysis are deaths rate of political violence of 1973–77, upper 20% income share (as a measure of income inequality), energy consumption (as a measure of economic development), intensity of separatism, semirepressive regime, and negative sanctions per one million in 1973–77 and in 1968–72. In addition, there are two variables of land inequality: agrarian inequality and landlessness. For details, see Muller and Seligson 1987. Data are provided by Muller.

7. Least squares estimation often results in predicted event counts that are less than zero. A predicted value that is negative is empirically meaningless in event count analysis. For example, an assertion that the number of deaths in country A between 1973 and 1977 is (− 1) is empirically and theoretically impossible.

8. If a prior occurrence of an event actually increases the probability of subsequent occurrence of similar or same event, the data are said to be *overdispersed*. When a prior occurrence of an event decreases the probability of subsequent events, the data are considered *underdispersed* (King 1989b).

9. In fact, when Gini land concentration is included in the analysis, it is not statistically significant either with or without outliers.

10. Muller and Seligson adopt the same argument (1987, 431). Meanwhile, there are many criticisms toward the use of Gini index as a single measure of land inequality. See, e.g., Brockett 1992; Midlarsky 1988, 1989; Prosterman and Riedinger 1987.

11. These countries are Argentina, Philippine, and Zimbabwe. The accumulated number of deaths from political violence for these three countries between 1973 and 1977 are 4,476, 3,236, and 3,491. The death rates per one million before adjustment are 176.33, 72.82, and 545.12, respectively.

12. The robust standardized residual (ϵ_i^*) is defined as the standardized residual (ϵ_i) divided by the squared root of the estimated dispersion parameter ($\hat{\sigma}^2$), that is, $\epsilon_i^* = \epsilon_i/\hat{\sigma}$. The ϵ_i is defined as the Poisson residual (u_i) divided by its estimated standard deviation: $\epsilon_i = u_i/\sqrt{\hat{\lambda}_i}$, where $u_i = y_i - \hat{\lambda}_i$, and $\hat{\sigma}^2 = (n - p)^{-1} \Sigma\, [(y_i - \hat{\lambda}_i)^2/\hat{\lambda}_i]$, where n is the total number of observations and p is the number of independent variables plus one. See McCullagh and Nelder 1989, 200–206; Winkelmann and Zimmermann 1992.

13. Zimbabwe is not included in the following analysis due to missing values on landlessness and agrarian inequality.

14. Cameron and Trivedi suggest that a one-tailed test is appropriate, because it is testing whether α is greater than zero (1986, 43).

15. The results of Table 2, columns 1–2, are generated by the computer program COUNT (King 1991), which employs the NEGBIN I parameterization.

16. The robust standardized residuals for the three countries are 2.58, 3.08, and 2.79, respectively. None of the rest of the robust standardized residuals is greater than 2 in absolute value.

17. For a review of these arguments, see Lichbach 1989; Snyder 1978.

18. The calculation of predicted values will depend on certain assumptions that should be made explicit. For an arbitrarily selected observation, the LOLS model can be written as $ln(Y + c) = X\beta + \epsilon$, where c is a constant added to the dependent variable prior to taking logarithms. Solved for the original dependent variable, Y, this becomes $Y = \exp(X\beta + \epsilon) - c$, where exp( ) indicates exponentiation. Since predicted values are just estimates of the expected value of the dependent variable, given the independent variables, X, we examine

$$E(Y|X) = E[(\exp(X\beta)\exp(\epsilon) - c)|X].$$

By the usual assumption that X is independent of the error, ϵ, and by properties of conditional expectations, this can be further simplified to

$$E(Y|X) = \exp(X\beta)E(\exp(\epsilon)) - c. \qquad \textbf{(10)}$$

Because an exponentiated normal random variable with a zero mean has an expected value of one, if ϵ is assumed to be normally distributed with a zero mean, this expression can be simplified still further to $E(Y|X) = \exp(X\beta) - c$. Thus, whether predicted values are estimated by this last expression or equation 10 depends on assumptions about the error term in the original model. For simplicity of exposition here; and for our LOLS estimations on death rates later, we assume a normally distributed error with zero mean. Although this is a common assumption in OLS, it is not always an appropriate one and may be particularly questionable for some models on raw counts.

19. Note that in his Table 1, column 1, Wang correctly reports an LOLS estimate of .98 for semirepressive regime. This value was misprinted in Muller and Seligson 1987, equation 1.3: the coefficient of 1.23 on intensity of separatism was repeated for regime repressiveness, the variable below it in the equation.

20. The addition of an increment of 1.0 before logging has been the standard procedure in analysis of political conflict

event counts since such data began to be collected for large numbers of countries in the mid-1960s. (A few prominent examples prior to those cited by Wang for the 1980s are Russett et al. 1964; Gurr 1968; Hibbs 1973; and Snyder and Kelly 1977). Tufte describes the addition of 1.0 prior to logging as "the most common procedure" for overcoming the problem of zero values (1974, 109).

21. In the interest of space, we examine only this single bivariate relationship, though it should be understood that similar effects will occur for other variables in a multivariate model. Thus, a full picture of these effects would encompass additional bivariate plots, as well as a series of partial plots controlling for other variables appearing in the model.

22. This step-level representation may appear vaguely similar to what are sometimes called sample selection or hurdle models (King 1989a). In fact, they are quite different. A hurdle model permits the causal process accounting for a first death to differ from the process responsible for the accumulation of additional deaths. A step-level representation, on the other hand, would require a single causal process to account for a very large interval at the first death, a relatively small interval at the second, and progressively larger intervals thereafter.

23. Of course, the amount of distortion introduced by a fractional constant is necessarily a matter of degree. There is little difference between a constant of 1.0 and, say, .999. Thus, in a strictly technical sense there is still some arbitrariness in choosing among all the infinitesimally different values in the near vicinity of 1.0. However this amounts to picking extremely fine nits, since no one would claim these to make any difference in estimation outcomes.

24. To ensure that our procedures followed Wang as closely as possible, we used the same data and the same computational software. We are reasonably confident of these procedures, since we were able to reproduce all parameter estimates in his Table 2, column 1, to at least the second significant digit. Residual differences may be due to different treatments of the energy consumption per capita measure. We found its magnitudes too large in relation to other regressors to permit estimation, so we moved its decimal two places to the left. We are uncertain how Wang treated this problem.

25. Our previous estimations have censored death rates above 50 per million at 50 in acknowledgement of the inaccuracies of death reports in highly violent societies and also to reduce the influence of extreme cases (Boswell and Dixon 1990; Muller and Seligson 1987). Although we followed this practice in constructing the Figure 1 plots, we do not do so here in order to enhance the comparability between EPR and LOLS estimations. We shall return to this procedure and its justification.

26. The sample size is limited by the availability of information for income inequality. We shall use new information to increase the sample size to 66. Here, however, we rely only on the 62 countries available to Wang.

27. We consider a more directly comparable specification shortly.

28. Our collective experience with different measures of political violence and of economic development suggests that significant positive effects emerge only in change models that include lagged measures of violence (see Table 6 below, and Boswell and Dixon 1990; Muller and Seligson 1987). For an explanation of this rather perplexing result and an alternative formulation that eliminates it, see Boswell and Dixon 1993.

29. Wang correctly points out that the Gini index has been widely criticized and we are not persuaded that this weighting operation adequately corrects its flaws (Brockett 1992; Midlarsky 1988, 1989; Muller, Seligson, and Fu 1989; Prosterman and Riedinger 1987). Accordingly, our analysis of this variable should not be interpreted as an endorsement of its validity.

30. It would be equally revealing to compare EPR and LOLS using death rates, our preferred dependent variable. However, all of the EPR algorithms with which we are familiar are constrained to dependent variables expressed only in integer values.

31. The estimated EPR relationships are defined by

$$\hat{Y} = \exp(.892S + .644R + .360N + .322P - .474 + .037I),$$

where S and R are both valued at 1 or 0 to symbolize the presence or absence of separatism and semirepressiveness, respectively; N and P are the means of log transformed negative sanctions and population; and I varies between 30 and 70 which is the observed range of inequality. Using the same notation, the LOLS functions are given by

$$\hat{Y} = \exp(2.004S + .1391R + .758N + .685P - 8.684 + .071I) - 1,$$

where 1 is an adjustment for the constant increment.

32. This may appear puzzling given that LOLS was found to return systematically larger estimates than EPR. The explanation is in the large *negative* LOLS constant which has the effect of adjusting the predicted values downward.

33. The choice of 50 is somewhat arbitrary. Our experiments with different censoring points shows that the value chosen makes little difference in the results. However, the implementation of a censoring point necessarily introduces some bias that will reduce the size of subsequent parameter estimates.

34. These four countries are Austria, Guatemala, Iran, and Senegal. A summary measure of inequality for Austria was obtained from Simpson (1990); measures for the others are from Muller 1988a. All other inequality measures used are reported in Muller and Seligson 1987.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN MARCH, 1994

The following articles, research notes and controversies have been scheduled for publication in a forthcoming issue.

Lucius J. Barker. "1993 Presidential Address. Limits of Political Strategy: A Systemic View of the African American Experience."

Barry Ames. "The Reverse Coattails Effect: Local Party Organization in the 1989 Brazilian Presidential Election."

David P. Baron. "Electoral Competition with Informed and Uninformed Voters."

Robert C. Bartlett. "Aristotle's Science of the Best Regime."

William J. Dixon. "Democracy and the Peaceful Settlement of International Conflict."

Michael W. Giles and Kaenan Hertz. "Racial Threat and Partisan Identification."

Jeffrey C. Isaac. "Oases in the Desert: Hannah Arendt on Democratic Politics."

David C. King. "The Nature of Congressional Committee Jurisdictions."

Arthur Lupia. "Shortcuts versus Encyclopedias: Information and Voting Behavior in California Insurance Reform Elections."

Walter R. Mebane, Jr. "Fiscal Constraints and Electoral Manipulation in American Social Welfare."

Richard Nadeau, Richard G. Niemi, and Timothy Amato. "Expectations and Preferences in British General Elections."

Joshua Parens. "Multiculturalism and the Problem of Particularism."

Kaare Strøm. "The Presthus Debacle: Intra Party Politics and Bargaining Failure in Norway."

George Tsebelis. "The Power of the European Parliament as a Conditional Agenda Setter."

Jean-Pierre Benoit and Lewis A. Kornhauser. "Social Choice in a Representative Democracy." A Research Note.

Wendy Rahn, John H. Aldrich, and Eugene Borgida. "Individual and Contexual Variations in Political Candidate Appraisal." A Research Note.

ERRATUM

In "Messages Received: The Political Impact of Media Exposure" by Larry M. Bartels, June 1993, page 270, the graphs contained in Figure 1, Summary of Estimated Effects of Television News Exposure, and Figure 2, Summary of Estimated Effects of Newspaper Exposure, were reversed.

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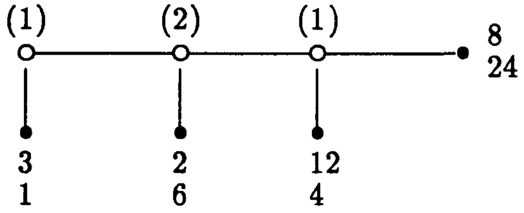
POLITICAL THEORY

Knowledge, Belief, and Strategic Interaction. Edited by Cristina Bicchieri and Maria Luisa Dalla Chiara. New York: Cambridge University Press, 1992. 413p. $64.95.
Paradoxes of Belief and Strategic Rationality. By Robert C. Koons. New York: Cambridge University Press, 1992. 172p. $49.95.

These two books consider a number of foundational issues in game theory having to do with common knowledge and rationality. Robert Koons is concerned with these issues primarily in relation to equilibrium concepts—such as subgame perfection—that rely on the notion of backward induction. His running example is the chain-store game, although his analysis is just as transparent in the following version of the centipede game:



Here, (j) refers to a decision node for player j; and player 1's payoffs are atop player 2's at the end nodes. It is easily seen that there is a unique subgame perfect equilibrium, namely, where both players play "down" at every opportunity. This game is commonly viewed as a "dynamic" counterpart to the prisoners' dilemma, in that the unique equilibrium outcome is inefficient: here both players would be better off by both playing "over" at every opportunity. However, it also serves to illustrate the following "paradox:" the theory predicts that player 2's decision node will not be reached. Hence, if this node is *in fact* reached, something must be wrong with the theory. So suppose player 2 interprets 1's move "over" as indicative of 1's irrationality, where such irrationality implies 1 will move "over" subsequently, as well. Player 2 would then also move "over," thereby receiving a payoff of 24, as opposed to 6. Thus, there is a benefit to be had by player 1 from player 2's believing 1 to be irrational, since player 1 would then receive either 8 or 12. But then player 2 should not believe that 1 is irrational after all, since 1 was merely attempting to maximize payoff by choosing "over;" therefore, 1 will, in fact, move "down" subsequently. But then, moving "over" initially *really is* irrational; and so we have the following quandary: initially moving "over" is irrational if and only if it is not irrational. (An appeal to the weaker notion of Nash equilibrium does not help matters, since the unique Nash equilibrium outcome still has player 1 moving "down" initially.)

Two sources for this paradox can be identified. First, irrationality is presumed to be persistent, so that an irrational move today implies an irrational move tomorrow. If, instead, irrationality is thought of as *unpredictability,* then there is no reason that player 2 should necessarily believe 1 will play "over" subsequent to a move "over" initially. The second is the presence of an out-of-equilibrium decision node, wherein the "what if" reasoning implied by backward induction is directly contradicted by the subsequent full expression of the theory. There are numerous ways out of this predicament, one of the more popular being the modification of the environment to allow for some uncertainty concerning player 1's payoffs. For example, if with some small probability, 1's payoffs were, in fact, the same as 2's in the game described, then there would exist an equilibrium of this (now) incomplete-information game where, regardless of 1's payoffs, 1 sometimes moves "over" initially.

This equilibrium involves non-uniform mixed strategies (i.e., different probabilities on equally good actions), and Koons's response to this maneuver is to argue against such strategies for games in general. The basis for this argument is the principle of insufficient reason: there is nothing in the description of the game to implicate one mixed strategy over another when a player is indifferent between two strategies. Hence, there is no way for other players rationally to deduce what mixed strategy an indifferent player will adopt. Koons then demonstrates how limiting the players to uniform mixing generates a probabilistic version of the paradox. (A relevant counter-argument is given by the "purification" results attributable to John Harsanyi: a mixed strategy equilibrium of a game is the limiting equilibrium behavior in certain incomplete-information games when the payoff uncertainty vanishes. Koons's counter-counter-argument is that the purification results require a specific form of incomplete information—for example, continuous distributions—and, hence, are not generally applicable.)

In this manner, Koons substantiates his claim that the paradox is unavoidable using conventional methods. His chosen analysis employs mathematical logic and begins by identifying similarities between the paradox in question and other logical antinomies such as the paradox of the liar (i.e., sentences that read "this sentence is not true"). He then modifies recent solutions to these antinomies to fit the current context. (Note that applications of formal logic constitute the bulk of this book. Hence, a healthy understanding of such analysis is useful in absorbing some of the more intricate results.) Essentially, these involve treating the paradox as a "blindspot" afflicting both participants: player 1 does not know whether 2 will move over if 1 moves over initially, and player 2 does not know that player 1 does not know this. Thus, "neither can predict what the other would do, even given full information about the other's data and utility function" (p. 127). Koons concludes by conjecturing that any reasonable theory would involve the players behaving "as if" they occupied a different environment, one where these blindspots do not occur.

Two of the contributions to the Bicchieri and Dalla Chiara volume also speak to this paradox but deal directly with the typical common-knowledge assumptions made in games—that is, assumptions of the form "Player 1 knows x, player 2 knows x, player 1 knows that player 2 knows x, etc., ad infinitum). Cristina Bicchieri shows that if the assumption that the players' rationality is common knowledge is weakened so that the regress terminates at a specific level, then the subgame perfect equilibrium is consistent. However, if the players are (at least) one-level "more rational," we return to the paradox. Conversely, Philip Reny argues for maintaining the assumption on the players' rationality and weakening the assumption that the equilibrium theory *itself* is common knowledge. For example, in the described game, it may not be common knowledge that player 1 understands backward induction. Hence, a move "over" initially is not necessarily inconsistent. (The paper by Joseph Kadane and Teddy Seidenfeld also deals with perfect-information games like the centipede game and argue for backward induction merely as a reasoning process for *identifying* optimal [i.e., Nash] strategies, rather than as part of the theory itself.)

These issues of common knowledge and rationality form the heart and soul of almost the entire collection. For example, there are papers by Michael Bacharach on acquiring common knowledge, Ariel Rubinstein on "almost" common knowledge, and Ken Binmore and Hyun Song Shin on common knowledge and computational complexity, as well as a variety of papers on possible worlds, counterfactuals, and the like. Most of this work follows Koons in adopting the tools of mathematical logic, and a working knowledge of this type of analysis is helpful.

Finally, the papers of Jordan Howard Sobel and Wlodzimierz Rabinowicz study an equilibrium concept for normal-form games to which rational players might be able to reason. The main differences from the Nash concept are that (1) players can only mix uniformly and (2) *all* best responses receive positive probability—the rationale for the latter being, again, the principle of insufficient reason. As an application, consider the normal-form representation of the described centipede game (or even the *agent* normal-form). When player 1 plays "down," player 2 is indifferent between playing "down" and playing "over" and hence, under this theory, must play each with probability one-half. But then, regardless of what player 1 would do subsequently, it is better for player 1 to move "over" initially, thereby eliminating the subgame perfect equilibrium from consideration. Two possible interpretations of this conclusion that I can come up with are that (1) backward induction *really is* unrealistic and (2) normal-form reasoning in extensive-form games is inappropriate.

University of Rochester JEFFREY S. BANKS

The Politics of Irony: Essays in Self-Betrayal. Edited by Daniel W. Conway and John E. Seery. New York: St. Martin's, 1992. 226p. $39.95.

This collection of essays is important because it addresses the crucial but elusive concept of irony. Each essay—and the collection as a whole—is first-rate, because irony in political thought and politics is investigated, rather than idealized. Indeed, by rethinking what irony is and does, the essays represent a provocative rethinking of political theory as an enterprise.

The editors, Daniel Conway and John Seery, frame the essays by turning two oft-repeated claims into questions. Does irony threaten politics by undermining the commitments and solidity of self that are ostensibly necessary to political action? What are the political implications of the apparent triumph of irony in contemporary academic discourse? These questions are pursued by essays in the history of political thought broadly construed—John Seery on Plato, John Traugott on Swift, Jane Brown on Goethe, Daniel Conway on Nietzsche, and Sara Blair on Virginia Woolf; William Connolly on interpretation, Jane Bennet on J. M. Coetze and the politics of resistance, Henry Kariel on (post)modernity, and Jean Elshtain and Richard Rorty on irony and liberalism.

But what is irony? The *Oxford English Dictionary*, ever the chaste definer, says that irony is "a figure of speech in which the unintended meaning is the opposite of that expressed by the words used." As a performance in texts and life, irony denotes "dissimulation" and "ignorance purposefully affected"; as a "condition of affairs," irony denotes the "unintended" to a degree that "mocks" intentions. Now, of course, irony carries broader connotations, which Seery and Conway associate with the unrelenting and self-referential skepticism that questions all ideals and commitments, that deflates intentions, that undermines certainties, that exposes the contingency of all representations.

Irony about ideals and authority, therefore, appears to threaten politics, as Jane Bennet argues, by diluting moral indignation while undermining the principles and bonds by which it is transformed into sustained political participation or even just episodic dissent. Ironical distancing from the self and principles thus seems bound to acquiescence to injustice or to outright cynicism, while in contemporary discourses, it seems to promote the smug self-righteousness of those who can claim to be in the know, whose deconstruction of narratives, ideals, and authorities in fact secures their own—albeit hollow—authority. Irony, linked to "nihilism" but also to the academic success of its agents, thereby comes to signify the dangers that theorists must thwart in Straussian terms, by securing the authority of nature as a horizon, or else in Habermasian terms, by providing the grounds for achieving self-reflective and rational consensus.

These essays pursue an alternative view of irony. It begins, as Norman Jacobson argued in *Pride and Solace*, with two claims held in tension: that politics originates in lack—the lack of an obvious, certain, or unitary common good—and that political thought to varying degrees originates in the effort to supply what is lacking. As Stanley Fish also argued, in *Self-Consuming Artifacts*, theorists thus appear as "bad physicians" (agents of what Job called "false comfort") or as "good physicians," who make palpable—but defeat—the yearning for completion and redemption in their readers. In Seery's reading of *The Republic*, then, Plato and Socrates promise—but intentionally fail to deliver—the transcendental standards that would rescue human beings from the cave, that is, the space of plurality, finitude, and therefore politics.

The result of this "turn" is to generate fruitful tension between yearnings and ideals and the irony that questions them. None of the writers here is therefore an "antifoundationalist," because, as Conway argues in his fine essay on Nietzsche, the death of god does not free us from relying on guarantors of meaning any more than insight into the conventionality of the "internalist" vocabulary of "the subject" frees us from needing and using it. As Connolly argues, representations of self, other, and world are indispensable, valuable, and costly but no longer transparent, objective, and neutral. The either/or of authority or nihilism, then, assumes that in the absence of assured guarantees, everything is worthless. But as Bennet argues in her wonderful essay on Coetze, a dogmatic commitment to ideals and their cynical repudiation are both symptoms of the resentment that squeezes out the space of politics.

On this alternative view, irony breaks the symbiotic marriage of idealism and cynicism. By reconceiving the longings that drive the one and the disappointments that make the other, irony creates a space for ideals to be held and understood differently. These essays, therefore, focus on the performative dimension in texts, to explore how theorists create and exercise their authority, simultaneously teaching about the political world, the forms by which they represent it, and the need for redemption that often binds an audience to authorities providing solace.

What are the political stakes in such performances? First, texts are political because they install the presuppositions that frame self-reflection and action. Irony is political insofar as it undermines their originary authority—as well as the authority of the ironist—to return readers to, or confront them with, responsibility to each other and their words. Second, in the world beyond the text, irony is a way to contest a political condition in which winners are those who project the greatest dogmatism in identity and belief (as Bennet puts it). Third, as Blair argues about Woolf, irony is a condition of entry for those excluded through ideologies of difference: Woolf gained a hearing through a performance that both affirmed and subverted—and hence revalued—dominant constructions of "the feminine."

The political assumption common in these essays is that the greatest danger in and to politics lies in any form of dogmatic identity and its sources—even as each acknowledges how assumptions or foundations are inescapable and valuable since otherwise, neither selves nor communities can exist. Seeking a political condition grounded in mutual recognition of this tension between contingency and what Nietzsche called *amor fati*, these essays project a political ideal of what Connolly calls "agonistic respect." That respect develops only as people learn the limits that justify their assertion but chasten their grandiosity. Those limits are the gift of the irony that draws a democratic ethos from its revaluation of life in the cave.

Still, this discourse of irony may be twinned with its transcendentalist and consensualist adversaries. Twenty-five years of political defeat and cultural reaction are reflected in various ways of displacing politics. One increasingly popular kind of theory idealizes deliberation to solve procedurally the social conflicts that appear intractable. The ironists here sometimes make culture a surrogate for politics as they defend an ethos to thaw the frozen. But unlike their adversaries, they tend to keep questions open, while sustaining the ability to pursue them in unexpected ways. Inclined to celebrate rather than cure contestation, those defending the value of irony keep the political spirit alive; for they protect the labor of the negative, which can bear the capacity for new beginnings.

New School for Social Research GEORGE SHULMAN

Beyond Individualism: Reconstituting the Liberal Self. By Jack Crittenden. New York: Oxford University Press, 1992. 230p. $38.00.

The Common Mind: An Essay on Psychology, Society, and Politics. By Philip Pettit. New York: Oxford University Press, 1993. 365p. $39.95.

The disagreements between liberals and communitarians, which are partially about the differences between holism and atomism, are now so well rehearsed that they have become more tiresome than instructive. What is needed is work that sifts these controversies with an eye toward uncovering and advancing some new theoretical project. *Beyond Individualism* and *The Common Mind* are two such books. In different ways, each moves the discussion of the nature of the self, social explanation, and related topics to a new and largely more interesting level of discussion. *Beyond Individualism* proposes a theory of "compound individuality" as an alternative to both liberal and communitarian understandings of the self. Drawing upon work in developmental psychology, Crittenden argues that we can have our liberalism so long as we leaven it with a pretty good dose of communitarianism. Interestingly, he does not see much point in the opposite strategy, that is, trying to make communitarianism more liberal. *The Common Mind* seems a more ambitious work, but this is partly because it covers a great deal more ground. Where the two works most clearly touch is in Pettit's defense of *holistic individualism*. This is a combination that does two things: (1) it defends *individualism* against collectivism or voluntarism against structuralism in accounts of how much intentionality matters relative to social-structural regularities; and (2) it sides with *holism* against atomism on the issue of how the self is constituted, especially the self's capacity for thought.

While the two books differ in a variety of ways, one of these seems particularly worth noting, namely, that Crittenden uses developmental psychology to make an argument *in* political theory, appropriating evidence from the work of such people as Lawrence Kohlberg, Carol Gilligan, and William Perry to forge a link between two very specific values, autonomy and sociality. Pettit, on the other hand, concludes with a chapter on political theory mainly to illustrate his theses about the psychology of the mind and the structure of explanatory theory. Most political theorists will probably find *Beyond Individuality* the more congenial, as well as more instructive, work.

Atomism is a term often used to describe aspects of the liberal understanding of the self that critics find particularly problematic. As an epistemological point, the controversy revolves around the question whether, as Pettit puts it, "we are individually dependent on our relations with one another for the possession of essential human capacities, in particular the capacity to think" (p. ix). Though they refine different propositions (Pettit's are more psychological, Crittenden's more developmental) and borrow from different literatures, Crittenden and Pettit agree that "integration and relationship" or "community with others" is a requirement of thought itself (*Beyond Individualism*, p. 73; *Common Mind*, p. ix). For Pettit, this means that holism is more accurate than atomism, for Crittenden, that sociality is a truer characterization of the self than self-sufficiency. As these comments signal, epistemological points shade rather quickly into more ontological ones. And at one level, one could read these two books as an analysis of how the self is constituted and what this formative process tells us about the self's capacity for development, moral maturation, and reflexivity. But that does not quite get it right. In fact, these sorts of topics are central to Crittenden's inquiry but less so to Pettit's. *The Common Mind* begins with an account of the psychological makeup of a knowing subject (part 1), moves to the implications of intentional psychology for rival explanatory strategies (part 2), and ends with a defense of holistic individualism. So Crittenden and Pettit cross paths, but they neither start from the same point nor head in the same direction.

While often technical, the two hundred pages of *The Common Mind* that bring us to holistic individualism are written in a clear style that will not tax the patient reader. Nor do I find fault with Pettit's use of holistic individualism to identify the criteria for a good explanatory theory. At a time when there is often much pushing and shoving among rival approaches, his defense of a sort of "explanatory ecumenism" that freely appropriates from a variety of styles of explanation is refreshing and—it seems to me—on the whole, persuasive. But the doctrine of holistic individualism seems neither very helpful nor particularly interesting when Pettit deploys it to discuss the nature of political theory and liberalism. He claims that holistic individualism can help liberals abandon their hostility to a more republican conception of freedom by allowing them to drop atomism but still celebrate the individual as an intentional agent. He chides liberals for placing too much emphasis upon the "quantity of non-interference" and not enough on the "quality of protection against interference" (p. 310). But this discussion miscasts and greatly oversimplifies some key issues—as, for example, when liberals are characterized as seeing every form of state activity as an interference. There are plenty of liberal theorists to whom one can look for a criticism of that simple formula (see David Spitz, *The Real World of Liberalism*, 1982). And it is simply wrong to say that liberals have not had much to say about the quality of protection necessary to enjoy freedom (see Richard Flathman, *The Philosophy and Politics of Freedom*, 1987). Worrisome about this sort of oversimplification is that it is (apparently) encouraged by holistic individualism. If true, it casts a shadow over the earlier parts of the book, as well. *The Common Mind* is a better psychology—even sociology—than it is a political theory.

Beyond Individualism uses developmental psychology to evaluate liberal and communitarian images of the self. Because Crittenden's political questions always control his use of developmental psychology, his book better demonstrates how psychology can enrich political theory. In any case, what makes this a worthwhile and very satisfying study is that Crittenden always has his eye, so to speak, on the prize. The prize, in this instance, is a theory of the self that will combine autonomy *and* sociality. Crittenden carries out several analyses to show how this is possible. The first argues that communitarians are right in emphasizing the importance of relationships, attachments, and social interactions to the formation of the self. But at the same time, he sides with liberals in their insistence that these formative processes cannot exhaust who someone is and that a capacity for self-reflection and the conditions that develop that capacity are as critical to autonomous selfhood as is community membership. Somewhere between the atomistic self of liberalism and the membership self of communitarianism, Crittenden reasonably suggests, is a more attractive self.

Compound individuality is Crittenden's not entirely felicitous phrase for this improved theory of the self that evidences both autonomy and sociality. The phrase itself is suggested by the psychological theories that characterize self-development as a process of cognitive and ego development, where the basic structure of earlier stages are incorporated or integrated into later ones: "Thus, the individual is a compound self or a *compound individual*" (p. 45). The compound individual has principles *and* understands that principles too rigidly held can damage relationships with others, values the marketplace of ideas *and* aspires to a more inclusive perspective in which the clash of opinions is superseded by a shared understanding. If one were to use a postmodernist idiom, one might say that here is a self who is a compound of identity (sociality) and difference (autonomy).

Judgments will vary about the success of Crittenden's project. My own preference would be for an informal norm to the effect that no proposal for transforming the political into a more intimate, integrative experience can be advanced prior to addressing the problem of size in a modern democracy. The compound individual is very plausible as a psychological type, less plausible as an image of the democratic citizen. Nonetheless, *Beyond Individualism* shows us that there are other choices besides liberal atomism and communitarian nostalgia. It is something that we already should have known, but Crittenden is right to have reminded us.

University of Florida ALFONSO J. DAMICO

Dimensions of Radical Democracy: Pluralism, Citizenship, Community. Edited by Chantal Mouffe. New York: Verso, 1992. 254p. $64.95 cloth, $18.95 paper.

Democracy and Complexity: A Realist Approach. By Danilo Zolo. Pennsylvania: Pennsylvania State University Press, 1992. 202p. $35.00 cloth, $16.95 paper.

These two books address, from different perspectives, a central issue of contemporary democratic theory: How are we to understand democratic politics at a time when the society that democracy has to govern appears increasingly fragmented into a multitude of groups, each having its own distinct identity and issuing political demands based on that identity? How can we hold on to the democratic ideals of citizenship and the popular will in the face of such radical pluralism? The answers given are, however, very different. Danilo Zolo offers us a thoroughgoing "realist" interpretation of contemporary politics that is extremely pessimistic about the possibilities of anything we would recognize as democracy. The contributors to Chantal Mouffe's volume, in their different ways, all aspire to greater democratization. They are attracted by the idea of citizenship as representing something more than a formal legal status, and they want to show that it has continuing relevance in modern liberal societies. So we have here the makings of an interesting debate.

Of the two books, Zolo's is by some considerable margin the more impressive. Its intellectual antecedents lie in the Italian tradition of political realism which runs from Machiavelli through Pareto and Mosca, but to this it adds eclectic borrowings from a wide range of sources in recent social and political theory. These are deployed to attack, on the one hand, empirical political science and, on the other hand, normative theory with universalist aspirations, such as that found in the work of Rawls. Zolo argues that the complexity of modern societies and the resulting fragmentation of standards of truth doom both of these enterprises. Instead, we should begin with a view of politics as achieving "the selective regulation of social risks." We accede to political authority because this serves to reduce the uncertainties of social life, which, in the contemporary world, always tend to increase.

This leads to a view of democracy that we might call neo-Schumpeterian. Zolo whole-heartedly endorses Schumpeter's famous attack on "the classical doctrine of democracy" but then goes on to argue that Schumpeter's own elite-competition model has been overtaken by recent developments. Parties no longer genuinely compete to attract the popular vote: instead, they collude with each other and establish client relationships with groups outside the political sphere. The electorate no longer possess even that minimum level of political rationality needed to make the competitive model work. Their political experience is constructed for them by the mass media, which is most effective when not engaged in overt propaganda. The resulting system, Zolo argues, no longer deserves to be called a representative democracy: "liberal oligarchy" would be more accurate.

Having delivered this indictment, Zolo's book comes to a sudden halt. He is hardly enchanted by the system he has described, but he appears to lack the resources to propose an alternative. Yet this disability is self-inflicted. He has ruled out, on epistemological grounds, empirical evidence that might, for instance, challenge his account of the effects of the mass media. And his attack on normative theory overlooks the fact that the systems he is describing are held together, in part at least, by the democratic principles espoused by their members, politicians, and voters alike. (One of the less helpful of Zolo's borrowings is a form of functionalism that seeks to explain the workings of the political system without reference to the aims and intentions of the actors themselves.) That is why Rawls's ambition to defend a normative theory by reference to the shared public culture of liberal democracies is not absurd.

One might say that Zolo, having written his *Prince,* ought now to attempt his *Discourses*. Yet this is a challenging book for those inclined toward the radical democratic view taken up by most of the contributors to Mouffe's collection. Zolo lays his finger on the central difficulty: "What this radical-democratic vision appears to me to lack most of all is a perception of the variety, particularism and mutual incompatibility of social expectations in non-elementary societies. It fails to consider the structurally scarce nature both of social resources and of the instruments of power responsible for the allocation of politically distributable resources" (p. 70). In other words, some, at least, of the conflicts thrown up by a fragmented society are zero-sum; and simply to encourage higher levels of political participation by hitherto excluded or passive groups does nothing to resolve this problem.

The challenge for would-be radical democrats is to show how it is possible both to respect the separate identities of the many groups that emerge in such a society and, at the same time, to arrive at collective decisions that are recognized as legitimate by all these groups. How can we be authentically female, black, gay, French-speaking, and so on but also equal citizens identifying with the laws and policies of the state? The challenge is a formidable one; and it is tempting to escape it by taking refuge in diffuse and obscure formulations in which Mouffe's book, unfortunately, abounds. Here, for instance, is the editor herself:

> The creation of political identities as radical democratic citizens depends therefore on a collective form of identification among the democratic demands found in a variety of movements: women, workers, black, gay, ecological, as well as in several other "new social movements." This is a conception of citizenship which, through a common identification with a radical democratic interpretation of the principles of liberty and equality, aims at constructing a "we," a chain of equivalence among their demands so as to articulate them through the principle of democratic equivalence. (p. 236)

If this means anything at all, it suggests some spontaneous tempering of conflicting group demands in the name of democracy. But why and how?

Not all the contributors to Mouffe's book are so evasive. Candid recognition of the conflict between personal identity and citizenship can be found in the chapter by Jean Leca, who draws attention to the breakdown of a common culture (in France, especially) and the increasing difficulty of establishing cultural communication between different groups (ethnic, regional, etc.), and in an elegant essay by Michael Walzer, who praises the rise of civil society as a sphere of free association but sees that it cannot fully substitute for democratic citizenship on the national scale. A robust form of republicanism is defended by Sheldon Wolin in the concluding chapter, and Mary Dietz endeavors to make this stance more appealing to feminists (though the constructive part of the argument remains somewhat undeveloped). Another robust republican, Hannah Arendt, is discussed in a helpful chapter by Maurizio Passerin d'Entreves: d'Entreves perhaps underestimates the distance that separates Arendt from the contemporary politics of identity.

One theme that is missing from Mouffe's book is the idea of deliberative democracy, defended recently by Joshua Cohen, James Fishkin, and several others. If we are to acknowledge social complexity without falling prey to the pessimistic conclusions of Zolo's realism, it is to the possibilities of democratic dialogue between competing groups that we must surely look.

Nuffield College, Oxford DAVID MILLER

Citizens and Statesmen: A Study of Aristotle's Politics. By Mary P. Nichols. Savage: Rowman & Littlefield, 1991. 233p. $50.00 cloth, $19.95 paper.

The Public and the Private in Aristotle's Political Philosophy. By Judith A. Swanson. Ithaca: Cornell University Press, 1992. 244p. $32.95.

These two studies—both eminently worth reading—have several points in common. Both Mary Nichols and Judith Swanson are sympathetic readers of Aristotle who seek to make sense of his texts without denying their difficulty and ambiguity. Both acknowledge the strangeness of those texts as seen from any modern point of view, but both presuppose—and then go on to show—that these texts are nevertheless pertinent for our own thinking about politics. Both avoid the sort of anachronistic, simplifying, and inevitably condescending reconstruction of Aristotle as a systematic philosopher that characterizes both Anglo-American analytic philosophical studies and the critical dismissals of Aristotle from historicist and poststructuralist perspectives. Biographically, both authors express gratitude to Joseph Cropsey in their prefaces. Thematically, both are concerned with the problem of how to understand what Aristotle means by including, as an integral part of his political theorizing (both in *Ethics* 10 and *Politics* 7), the apparently antipolitical proposition that the political life cannot be ranked as highly as the philosophical or contemplative life; and both respond to this question in novel and interesting ways.

But the answers the two books give to this interpretive puzzle are diametrically opposed to one other. For Swanson, Aristotle's explicit preference for philosophy is meant to call attention to the dangers of the political life and the attractiveness of lives lived outside the public realm; whereas Nichols, places Aristotle's occasionally explicit preference for philosophy in the context of the *Politics* as a whole to show that this preference is not antipolitical but carries with it the message that philosophy is only possible for those who live a certain kind of political life. The interpretive controversy is a legitimate and necessary one, since Aristotle's brief discussion in *Ethics* 10 and scattered remarks in *Politics* 7 give no unequivocal answer to the question of how this infrequent privileging of philosophy over politics can be made to comport with his extensive defense of the political life, properly understood. Nichols and Swanson, in effect, give two quite different responses to the interpretive problem of what to do with Aristotle's ranking of philosophy over politics. Their interpretations open the way (as such interpretations should) to serious reflection on the politics of our own time. For Swanson, Aristotle's elevation of philosophy yields a defense of modern liberalism that can avoid the weaknesses of present-day rights-based or utilitarian political theories; for Nichols, reading Aristotle on this question can lead us to a more complex and less utopian understanding of politics than any modern political theory can, an understanding that—surprisingly—provides a powerful clarification and justification of modern democracy.

Stylistically, these are also two very different *kinds* of book. Nichols presents her novel and often controversial interpretations (e.g., that "polity" is the simply best regime, that the regime "according to prayer" is called the best only in irony, and that the best human life is impossible without political activity) in the form of a topic-by-topic and book-by-book commentary on the *Politics*. But her commentary is never so detailed that it fails to keep the whole text in view, thus opening her reading to question and revision by other readers of the *Politics*. All of us who teach the *Politics*, no matter how many or how few times we have done it before, will teach the book better and with more pleasure having read Nichols. The book should be of value both for experienced Aristotelians who need to see the text with a freshly invigorated eye and for beginning, nonspecialist teachers who are looking for interesting ways to problematize the *Politics* for their students.

Swanson's argument for her equally controversial thesis, on the other hand, ranges over Aristotle's corpus as a whole, connecting Aristotelian ideas and lines of argument that are generally not connected, rather than commenting intensively on a single text. Her claim is that—contrary to modern communitarian interpreters of varying political stripes—Aristotle's political philosophy establishes the supreme human value of a set of activities—including, but not limited to, philosophizing, the family, friendships, and the economy—that can flourish only within the private realm, activities that are inevitably threatened by the public. Her application of the private/public distinction to the text is less problematic than it may, at first glance, appear to be. While Aristotle does not speak of a distinction between private and public as such and thus does not himself explicitly assert anything about the value of either "the private" or "the public," Swanson's use of these terms as a way to open a dialogue with Aristotle is by no means an anachronistic imposition of modern concepts on a Greek text. One clear instance of a conception of "the private" in Greek philosophy is in Plato's *Laws* 739c–d, where the Athenian Stranger distinguishes between "what is called the private" and that which is said to be common, arguing that in the best regime everything would be common, including that which is by nature private, such as the eyes and the ears and the hands. There can be little doubt that Aristotle would both understand and reject the Stranger's claim; the modern formulation of the private/public issue would thus not be hopelessly unintelligible to the voice we can imagine speaking to us in the texts of Aristotle.

In essence, Swanson says that modern liberalism can borrow from Aristotle a defense of the private realm that is stronger than the familiar Lockean argument for the separation of private and public realms. There is a serious practical difficulty about this, however, in that Swanson's Aristotle would defend the private out of a deep mistrust of democratic public opinion, which he takes to be the principal obstacle to the development of human virtue. Thus, to accept Swanson's Aristotle on privacy, modern liberals would have to be convinced of the wisdom of abandoning their commitment to liberal *democracy*.

Be that as it may, Swanson's Aristotle is concerned with establishing a robust private world that might shield good and potentially good human beings against the corrupting influence of public opinion (pp. 207–08). There is, for her Aristotle, a reciprocity between private and public that is really a kind of rapprochement between the few and the many: the private sphere educates good rulers for the city, while the city, in exchange, protects the privacy and the private achievements of the more virtuous few. A central interpretive difficulty is how to make Swanson's picture square with two Aristotelian claims: (1) that a human life lived outside the laws is more likely to be beastly than angelic and (2) that for these laws to educate in virtue effectively, there must be not only widespread public consent but a significant degree of political participation, as well. When they are not actively appropriated by a majority of the citizens, the laws must fail to perform their primary function of supplying a moralizing authority that defines the city without reducing it to a unity. Swanson's response here is that the maintenance and revision of the laws and customs of any given polity are not of great concern because there is, for Aristotle, both a readily knowable natural hierarchy among human beings and a readily knowable substantive natural law to guide practical reason. (Aristotle, to most Straussians and to Nichols, seems to deny both of these things.) So long as the few who are best by nature rule, guided only by their independent judgment and by the precepts of a natural law that is embedded in the unwritten customs of every polity, there is no reason to be much troubled by those laws that make Athens distinctly Athenian.

The spin that Swanson puts on Aristotle is opposed to Arendtian and communitarian readings; but her own theoretical framework is not evident (to this reader, at least), though it seems to be in the direction of Oakeshott and possibly Voegelin, rather than Strauss. But this is never altogether clear, so it is difficult to ascertain what overall political theoretical conception supplies the context and motive for Swanson's unusual reading of Aristotle. This makes it hard to see any consistent narrative or discursive line to her argument as it moves from chapter to chapter to consider the virtues that her Aristotle thinks must be developed in the household (rather than the city), in the relation between master and slave, in the separate spheres of husband and wife, in the economic market, in friendship, and in philosophy. Her writing is sometimes awkward and stilted and conjures a very censorious and moralistic Aristotle, as though she were not yet fully comfortable with her texts. A provocative and wide-ranging study of this kind always risks relying too heavily on quirky readings of single passages taken out of context, on theory-driven readings of ambiguous passages, and on repeatedly exaggerated formulations of the point the author urgently wants us to hear. Swanson's work sometimes suffers from all these weaknesses, but her occasional stylistic infelicities do not obscure the novelty and the importance of what she has to say. There is something powerfully plausible in her assertion that Aristotle at least implies a conception of the private not as a protected space for pursuing one's own good in one's own way but as an opportunity for cultivating virtues. Swanson has read widely and well, and the book vividly calls attention to the various ways in which Aristotle's discussions of character and moral education regard public life with a much higher degree of suspicion than any ardent communitarian, ancient or modern, would display.

Mary Nichols is a master of the difficult rhetorical art of exposing her arguments to challenge without abandoning them, and because of this, her book speaks with an unusual authority that must arise from many hours intelligently spent reading and thinking and conversing about the meaning of the *Politics*. *Citizens and Statesmen* is a marvel of close reading combined with clarity and consummate accessibility. For Swanson's Aristotle, political life is neither ennobling nor challenging. For Nichols's Aristotle, it is both. Her study takes, as its point of departure, the dispute between "democratic" and "aristocratic" interpreters of the *Politics*, both of whom see in Aristotle a way of challenging modern liberalism by bringing to the fore qualities that liberalism seems to lack—democratic readers stressing Aristotle's focus on the importance of a political community composed of equal and participatory citizens, aristocratic readers, Aristotle's recognition of human inequality and of the importance of leadership by virtuous statesmen. Nichols's argument is that the two readings are each partially true and partially distorting; each exaggerates one element in Aristotle's political understanding, by depreciating either the extent to which Aristotle acknowledges human inequality (the typical failure of the democratic reading) or the extent to which he is aware of the dangers of falsely claiming superiority or overstating the degree of superiority of some to others (the typical failure of the aristocratic reading). Nichols's position is that good politics for Aristotle must accommodate the just claims of both the democratic many and the statesmanly few; to hold otherwise, she argues, is to court democratic or oligarchic despotism. Statesmen and democratic citizens are mutually dependent. Without some degree of democratic participation as a check on their powers, statesmen are likely to forget that they are humans subject to human limitations and incline to substitute tyranny for polity; without statesmanly leadership, citizens will fall into factional conflict and reduce the political community to civil war.

Similarly, philosophy and political activity are mutually supportive activities. Statesmanship involves learning the complexity of the world, learning what can be controlled and what is beyond control. By acting as statesmen and only by acting as statesmen, philosophers like Aristotle are in a position to understand themselves and so to make a step toward understanding the whole. The *Politics* itself, Nichols argues, involves statesmanship of this kind. Nichols is careful not to dissolve all difference between philosophy and statesmanship. (Unlike the statesman, Aristotle's chief interest, she says, is not establishing the best possible polis but understanding beings.) But there is no immediate royal road to wisdom for Nichols's Aristotle: the path to such understanding lies only through the city. Her account of the need for mediation and indirection in Aristotle's version of philosophic education recalls the Platonic Socrates' flight to the *logoi* (rational and rambling speeches, as opposed to algorithmic deduction or intuitive silence) and to self-understanding as a necessary stage of the human journey to wisdom, in spite of the fact that she, too, often insists on a picture of a rigidly aristocratic Plato to serve as a foil for her more flexible and democratic Aristotle. It is also the case that her reading needs to be tempered by readings like Judith Swanson's. I would add that, contra Nichols, the road through politics and self-understanding is not the only option Aristotle suggests for our indirect approach to the hidden truths of things. Potentiality, actuality and the other central elements of first philosophy can also be approached through the careful study of nature and the many natures—through what we call natural science. In the end, what matters most is that both Nichols and Swanson implicitly maintain still a third mode of indirect access to the things that are, the way of articulate and critical reflection on texts such as Aristotle's *Politics*.

Bryn Mawr College STEPHEN SALKEVER

The Quality of Life. Edited by Martha Nussbaum and Amartya Sen. New York: Oxford University Press, 1993. 452p. $65.00 cloth, $19.95 paper.

This plump volume records the proceedings of a conference held at Helsinki in 1988, under sponsorship of the UN's World Institute for Development Economics Research (WIDER). We are told on the fly sheet that WIDER's purpose "is to help identify and meet the need for policy-oriented socioeconomic research on pressing global and development problems, as well as common domestic problems and their interrelationships." It is not altogether easy to see how this conference and the edited volume before us answer to this set of concerns. But it is possible.

Imagine, if you will, a three-chambered legislature designed to deal with the very real problems of international development, carried forward in the midst of racial hatred, the widespread oppression of women, rapid destruction of irreplaceable natural resources, commonplace killing in the names of God, an AIDS epidemic menacing much of sub-Saharan Africa, and (more or less incidentally) the collapse of the Soviet world. The First House is directly open to people familiar with all this and is therefore a raucous babble of hatreds and affinities, obsessions and indifferences, short on elegance, long on jagged differences. Just upstairs is the Second House, limited to professional international bureaucrats and world-scale policy wonks. Its membership is familiar with the first-order consequences of world development politics, although each is carefully selected for a capacity to denature conflict by recourse to a language rich in "instrumentalities" and hedged in by "terms of reference." Mr. Lal Jayawardena, director of WIDER, sees the present project as an effort to bring together economists and philosophers "so as to lead up to results assimilable by policy-makers" (p. vi).

On top of the whole edifice is a Third House, peopled with distinguished philosophers and economists such as Martha Nussbaum, Amartya Sen, Hilary Putnam, Sissela Bok, Charles Taylor, Derek Parfit, Margarita Valdes, John Roemer, Paul Seabright, Michael Walzer, and B. M. S. van Praag. Most members of the Third House possess uneven and sometimes very thin news about doings in the First House and very little direct experience in delivering ideas to policymakers. Here, the common discourse centers largely on the resurgence of academic moral theory and the extension of certain results from social choice theory. Over recent generations, these discourses have grown richer in abstraction and poorer in human narrative—open, in principle, to any configuration of human needs but attentive to no specific real ones. If all these abstractions have to do with welfare and justice and if a few members can hear the din coming from below in the First House, why not see what can be done with some very abstract ideas to understand the sorts of very concrete problems raised by economic development and its failures. Without pretending to reach the First House with its answers, members of the Third House direct themselves to one another, and sometimes to the policy discourse of the Second House. *Quality of Life* is the result.

The recurring challenge is to retain sufficient universality that one is not merely replicating the relativized prejudices of First House discourse and yet to fold in strategically central features of that discourse, such as may serve to couple one's argument with real people's lives. If we recognize the richness of human needs and capacities, then we must judge that the usual welfare criteria of utilitarian philosophy, Rawlsian contract theory, and the thin ordinations of social choice theory fail to enlighten us greatly about what counts as quality in life. Mapping the possible futures of, for example, Nairobi's poor into standard international utiles, imaginary quantities of primary goods, or anything of the sort seems foolishly reductionist and is almost certain to inspire little confidence among Kenyan politicians or entrepreneurs.

The most interesting efforts (as would be imagined) attend to intermediate levels of abstraction. Amartya Sen's oft-discussed idea of *capability* is foremost among these:

> The capability of a person reflects the alternative combinations of functionings the person can achieve, and from which he or she can choose one collection. The approach is based on a view of living as a combination of various "doings and beings" with quality of life to be assessed in terms of the capability to achieve valuable functionings. Some functionings are very elementary, such as being adequately nourished, being in good health, etc., and these may be strongly valued by all, for obvious reasons. . . . Individuals may, however, differ a good deal from each other in the weights they attach to these different functionings . . . and the assessment of individual and social advantages must be alive to these variations. (p. 31)

This is a way of leaving discourse open to variation across cultures and economic levels yet attempting to provide a minimal vocabulary that travels across these lines of demarcation. It is complemented, in the collection, by more specific papers on such topics as the Swedish social democratic state's welfare indicators (Robert Erikson), a nice piece on medical quality-of-life issues (Don Brock), and a lively series of papers on gender issues (most notably from Onora O'Neill, Julia Annas, and Martha Nussbaum).

The volume also contains some important meta-ethical argumentation, among which the most notable is perhaps Charles Taylor's effort to develop an account of practical reason that is non-foundational without reducing itself to phenomenology. As Nussbaum notes in her commentary on the paper, Taylor's argument runs toward a discourse on development and like subjects that is "piecemeal rather than global," "very much rooted in the particularities of people's historical situations," and "biographical rather than abstract" (p. 236).

John Roemer offers an axiomatic development that leads to his "leximin rule" for budgeting scarce resources in attacking such problems as infant mortality. The rule tells us to begin investing in the country with the worst results so far and keep pouring in resources until its situation is no longer worse than the second-worst country. We then turn to that country and push its result up until it matches the third-worst country, and so on up the scale. The deduction is ingenious, although one cannot readily accept some of the axioms. Notably open to suspicion (as Paul Seabright points out) is Roemer's condition of monotonicity. This condition tells us that an increase in total resources available should never, ceteris paribus, lead to a decrease in the budget for any given country. The rub is that a budget increase might, when combined with monies redistributed from other countries, let us attack some large, lumpy, expensive problem in a given country to save hundreds of thousands of babies even at cost to monotonicity in Roemer's analysis.

It is nowhere evident that the agenda of world economic development as understood in either of the other houses will be directly altered by this volume. People worried about roads and dams will be dumfounded by such sentences as "One cannot infer from the central place in life—and midfare—of action and capacity that capability spreads across the entire space of midfare" (p. 23). I am, nevertheless, certain that the effort recorded here will help to draw philosophical and economic theory a little closer to the world of crying children and inflamed conflict.

Yale University DOUGLAS RAE

Economic Rights. Edited by Ellen Frankel Paul, Fred D. Miller, Jr., and Jeffrey Paul. New York: Cambridge University Press, 1992. 314p. $19.95 paper.

This is a collection of papers from a conference organized in San Diego in 1990 by the Social Philosophy and Policy Foundation (based in Bowling Green, Ohio). The papers address the nature and justification of various claims about economic rights. These include the right to private property (or, as contributor Randy Barnett calls it, "several property"); the right to freedom of contract (defended by Barnett, criticized by Daniel Hausman); the right of a creditor to have debts repaid (criticized in the context of Third World indebtedness by James Child); the right to secure what economists call "rent" from one's assets and talents (defended by Eric Mack); the right to education (discussed in the context of various economic models by John Roemer); the rights of disabled persons to be provided with meaningful and remunerative work (defended by Gregory Kavka); the right of the indigent to welfare assistance (defended, in separate papers, by Richard Arneson and David Copp); the right to participate in the democratic management of one's place of work (criticized by Jan Narveson); and the rights that would accrue to workers under market socialist institutions (criticized by N. Scott Arnold).

The authors do not waste any time discussing the distinction between economic and non-economic rights: they are too immersed in their particular subject matters to do that. The exception is a paper by Jonathan Macey. Macey laments the failure of the U.S. Supreme Court, since the *Lochner* era, to accord the same importance to property rights and freedom of contract as it accords to noneconomic rights such as freedom of speech. He considers a number of possible explanations for this, but the one that he favors has to do with the incentives that dominate judicial behavior in modern legal culture. In a hypothesis that will come as a happy surprise to many of my colleagues, Macey says that federal judges seek to maximize their prestige in the eyes of the legal academic establishment ("They want favorable articles written about their jurisprudential style in the law reviews") and that law professors are likely to frown upon judicial decisions that support market values. The more obvious philosophical reason for what he calls the "bifurcated treatment" of economic and other rights does not seem to have occurred to Macey. It is that while everyone agrees that there is at least a danger that existing legal rights of property are maldistributed from a moral point of view, such a possibility barely makes sense in the case of rights such as freedom of speech.

Few of the authors pay any attention to the notion of *a right*, as such. David Copp is the exception. Arguing that everyone is entitled to an adequate standard of living, Copp addresses the question of whether such "welfare entitlements" can properly be represented in the language of rights. He considers—and, quite correctly, dismisses—a number of analytic objections, such as those based on scarcity and on the discredited "choice theory" of rights. His discussion is oriented, instead, to the interest theory of rights, where rights-talk is taken simply as a way of representing the priority to be accorded certain important individual interests. He denies that rights have to be based on interests in liberty, though he is perfectly prepared to argue that being in a position to satisfy one's basic needs is a necessary condition of rational autonomy.

Copp's discussion is less satisfactory in his account of who bears the duties correlative to welfare rights. He maintains that these duties are incumbent on the state (by virtue of the responsibilities that flow from its powerful position in society) and only derivatively applicable to ordinary citizens: the requirement that citizens submit to taxation is to be thought of as part of their duty to support the state in discharging *its* duty. In other words, Copp does not derive the state's duty from any more basic duty incumbent upon individuals to help one another; indeed, he says that a poor person would have a right of assistance against a better-off individual only if the latter had the resources of a state. The trouble is that states—particularly welfare states—do not exist and sustain themselves by magic. Copp's argument is therefore incomplete without an account of individuals' political duty to bring a welfare state into existence and keep it in existence, a duty that simply must be grounded in some deeper and more primal obligation of justice to one's neighbors.

For philosophers, the authors in this volume are remarkably un-self-conscious about what counts as an argument for or against a position on rights. Some avail themselves of rather simple tu quoque strategies. Jan Narveson maintains that the very reasons which people adduce in favor of democracy at the state level (having to do with individual liberty) argue against democracy in the ordinary running of individual economic enterprises. Scott Arnold argues that market socialist institutions would be exploitative in exactly the sense in which socialists criticize capitalist institutions for being exploitative: inasmuch as they pursue egalitarian goals, socialist institutions are bound to deny better-endowed workers the full value of their contribution. (It is a pity that Arnold had not taken more notice of the discussion of exploitation in the recent work of his fellow symposiast, John Roemer. Roemer has made a convincing case that a discrepancy between contribution and return counts as objectionable exploitation only when it is made possible by an unjust distribution of property. But the relation between distribution and exploitation is barely considered in Arnold's article.)

Gregory Kavka's defense of the rights of the disabled is perhaps one step up from this, since he at least deploys the complex apparatus of Rawlsian contractarianism. But he does not do so in a thoughtful way, that is, in a way that would encourage the reader to consider why argument structures of this kind count in favor of the propositions they are adduced to support. His strategy, no less than that of his fellow authors in this volume, seems to be: (1) stake a political claim, (2) dredge up anything in contemporary philosophical discourse that might conceivably count in its favor, and (3) admit to no hesitations or ambivalence lest the position crumble. Thus, Kavka argues, plausibly enough, that persons behind John Rawls's veil of ignorance would prudently ensure themselves, in their choice of social principles, against the worst outcome of being both disabled and cut off from the economic life of mainstream society. But he seems disinclined to reflect on the difficulties that there may be in applying the underlying logic of contractarianism to the case of the disabled. Contractarian argument gets its foothold in political experience from the fact that we all have to come to terms with others in society who are in a position to make life difficult for us. But what if someone's disability prevents him from posing any sort of threat to others? What if the others can afford to ignore him (as they can afford to ignore, e.g., people who live in other continents)? Then he cannot base a moral claim on the *necessity* of their coming to terms with him: he must base it, instead, on his *moral right* to be dealt with on terms of equality. Though that right may be expressed in contractarian jargon, it is pretty clear that its real moral foundation must be sought elsewhere.

In general, there is a great danger, when writing on economic issues, that one will take the political positions one is defending to be more important than any deep and considered reflection one can offer on arguments and methods—as though the future of the West depended on how much rhetoric Jan Narveson could marshal in defense of capitalism or as though the plight of the homeless depended on David Copp's not yielding an inch to the libertarians. The articles that Ellen Paul and her fellow editors have assembled in this volume are certainly stimulating and provocative. I imagine they would provide a fine starting point for a semester's discussion of economic rights in a graduate seminar. All the same, what one misses is a sense that anyone is willing simply to explore, in a *philosophical* way—with all the doubts and difficulties that answer to the intractability of the subject matter—the complex connections between property, agency, economy, responsibility, justice, and human vicissitude. No one seems to have time or patience for that: everyone is too busy rushing to the defense of the bottom line.

University of California, Berkeley JEREMY WALDRON

Time, Labour, and Social Domination: A Reinterpretation of Marx's Critical Theory. By Moishe Postone. New York: Cambridge University Press, 1993. 424p. $49.95.

Long in the making, Moishe Postone's magnum opus has arrived at an appropriate moment. Marxian social analysis has fallen on hard times, dragged down by the welcome demise of the quaintly termed "formerly existing socialist societies." But the crises of capitalism remain unresolved and, in some respects, deepen. Postone's aim is, through a critical reconstruction of Marx's theory, to demonstrate that these crises cannot end without fundamental change.

It is clear that traditional Marxism as a critical social theory with emancipatory intent is inadequate. Among the phenomena of the last half-century that escape its ambit are the development and recent crises of state-interventionist postliberal capitalism, the rise and fall of so-called socialist societies, and the emergence of new social movements as a response to the appearance of new economic and environmental problems on a global scale. Of all the strands of twentieth-century Marxism, only that associated with the Frankfurt School is even a candidate for offering a satisfactory approach to these questions. Structuralist Marxism was always something of a contradiction in terms and, in any case, was too influenced by a narrow Parisian perspective. And Analytical Marxism (on which Postone has some telling footnotes) is an attempt to rethink Marxism through a methodological individualism that is quite alien to the mainspring of the Marxist tradition. The Frankfurt School was at least equipped to grasp the dynamic of the Marxian analysis: it failed in the lack of any sufficiently materialist grounding in its approach, in what has been called its "economic deficit."

It is this deficit that Postone aims to make good, and he has largely succeeded. Drawing heavily on Marx's *Grundrisse*, Postone develops an account of capitalism that goes far beyond the market, private property relations, and bourgeois domination. For Postone, the essential point in the reinterpretation of Marx's theory lies in the distinction between a critique of capitalism that starts from the standpoint of labor, as traditionally understood, and a critique that is based on an analysis of the historically determinate nature of labor under capitalism. It is the capitalist industrial production process itself (and not merely market mechanisms or private property relations) that so structures social labor in modern society that it leads inevitably to alienation and domination. This focus on the commodity as a socially constituted, as well as a socially constituting, form of social practice permits an approach that is subjective, as well as objective, mediating the dichotomy between structure and action, meaning and material life. It is a starting point that allows at least an approach to explaining the destructive paradoxes of modern society: the continued production of wealth amid poverty, the negative effect on social organization of a technology that saves both labor and time, and the contrast between the theoretical ability of people to control their social environment and their practical subjection to abstract and impersonal forces outside their control.

Postone's reconstruction of the core of Marx's critique of capitalism is (like Marx himself) most definitely sociology, rather than politics. His analysis suggests that the social limits to democracy in capitalist society lie deeper than simply the vast discrepancy between formal political equality and concrete social inequality and that, therefore, any postcapitalist democracy entails more than a simple combination of democratic political forms with the disappearance of private ownership of the means of production. For him, a genuinely democratic approach would also involve the constraints to democratic self-determination that are imposed by "the abstract form of domination rooted in the quasi-objective, totalizing, historically dynamic form of social mediation that constitutes capitalism" (p. 393). But Postone does not provide us (any more than does Marx himself) with an account of alternative, postliberal, collective forms of social organization outside the sphere of immediate production. We are left to imagine how workers might become collective commodity owners. This "political deficit" is one that Marx bequeathed to his followers and that remains unfulfilled. Postone's substantial work at least provides a solid socioeconomic basis on which to build future projections. He has produced the best analysis of Marx's mature socioeconomic theory since David Harvey's *The Limits to Capital* (1982). It is obviously the fruit of long years of research and lengthy meditation. It is the kind of work that is well worth waiting for.

University of Kent, Canterbury DAVID MCLELLAN

Political Liberalism. By John Rawls. New York: Columbia University Press, 1993. 401p. $29.95.

John Rawls, the outstanding political philosopher of the century, has here compiled, mostly in considerably revised form, the lectures that contain his thinking about justice since *A Theory of Justice* (1971). In that most influential book, he presented an account of social contract theory in which principles of social justice are arrived at by people thinking in an "original position"—in which they have no knowledge of particulars about themselves, their values and attachments, or their situation in the prospective just society. This idea and the arguments involved in working it out have yielded a vast torrent of commentary. Rawls's generous-spirited responsiveness to critics—long since obvious—is readily apparent in his new book, where he pays careful attention to many counterarguments and gives a great deal of credit to colleagues for helping him to clarify or to rethink aspects of his original theory. *Political Liberalism* is so much a sequel to *A Theory of Justice* that it is not readily intelligible to anyone not already familiar with the earlier work.

Rawls now says that his idea of a just or well-ordered society whose citizens endorse a comprehensive moral or philosophical conception of justice was unrealistic, because unstable. Why? Because, given human beings' use of their powers of reason, under conditions of freedom, they are likely to arrive at different moral, philosophical, or religious comprehensive conceptions of the good—views about how life should be lived. Given the existence of this "plurality of reasonable yet incompatible comprehensive doctrines" (p. xvi), which he calls "the fact of reasonable pluralism," he recasts his theory of justice as a political, rather than a comprehensive, one. As such, it applies to what he calls "the basic structure of society"—not to the whole of life, including its personal, associational, and familial aspects, which may be ordered in accordance with people's various comprehensive conceptions. Only such confinement of the theory to the realm of the political, Rawls thinks, will enable it to be subscribed to sufficiently to render the just society stable. He concentrates much of his attention on this task, since it requires him to recast or develop afresh, beside the conception of justice as strictly political, a number of other concepts central to his theory: a political conception of persons (as free and equal), a new account of an "overlapping consensus," and a more developed account of "political constructivism."

Given the emphasis on the fact of reasonable pluralism, it is not surprising that the central focus of *Political Liberalism* is the problem of toleration: How can persons with conflicting, though reasonable, systems of values live together in a just and stable society? Rawls argues that the political conception of justice that enables those who differ so profoundly to coexist peacefully must result from the citizens' *public* reason and that the theory of justice is the focus of an overlapping consensus, consistent with—and somehow (it is less clear exactly how) related to—the citizens' various reasonable comprehensive doctrines.

Rawls's focus on the liberties that constitute the conditions for toleration—freedom of conscience, speech, and association—virtually drowns out the redistributive part of his original theory. Apart from his continued insistence on a considerable extent of actual (not just formal) political equality, as a fundamental principle of justice, equality takes a back seat. The priority of liberty that Rawls always argued for has become a virtual monopoly. He supposes that there will be considerable social and economic inequalities in the just society. Yet both the difference principle and fair equality of opportunity—arrived at in *A Theory of Justice* through the "original position"—are no longer "constitutional essentials" and are relegated to the legislative stage of the theory (pp. 228–29, 237n.; but see also p. 340). In the last and longest lecture of the book, Rawls responds to H. L. A. Hart's objections to his arguments for the priority of liberty, depending heavily on his conception of persons as, above all, concerned with the development of their *moral* faculties. But I find myself asking (much as Hart did in his initial challenge) why one should assume that behind the veil of ignorance, one represents such a Rawlsian ideal of a human being, rather than one who (while by no means indifferent to morality) values above all else, say, the (costly) development of one's musical talents or the opportunities open to one's children.

Though his revised theory of justice is avowedly political, Rawls tends to look back—especially to the conflicts of the Reformation and the American Civil War—and rarely discusses the political issues that are often most controversial in modern pluralist societies. Thus, there are numerous discussions of slavery but none of substance on gender, race, or ethnicity. On families, Rawls is much less clear than in *A Theory of Justice*. Now they are both specified as part of the basic structure (p. 258) and said to be nonpolitical (pp. 10, 137, 195). But this, given Rawls's definition of the political, involves a contradiction in terms. Acknowledging his nonengagement of most current political controversies, he suggests that "once we get the conceptions and principles right for the basic historical questions, those conceptions and principles should be widely applicable to our own problems also" (p. xxix). However, he gives only a vague hint of how we might do this. We are left hoping that he will apply his theory to race, gender, ethnicity, and other pressing issues, in future writings.

In several of the lectures in *Political Liberalism*, Rawls clarifies aspects of *A Theory of Justice* that have been much misunderstood. Expanding on a point already made there—that the original position is a heuristic device—Rawls responds with great lucidity to those critics who have interpreted it literally, as if those in it were actual persons. He also makes the difference between the rational and the reasonable far clearer. While rationality is an agent's capacity for adopting ends and interests, deciding priorities among them, and finding the most efficient means to achieve them, reasonableness, as Rawls uses the term, is very close in meaning to justice and fairness. The result of this clarification is that there is now no chance that justice as fairness could be interpreted as a theory of rational choice.

As I understand it, the central argument of *Political Liberalism* leads to at least one of two problems. Rawls's recasting of justice-as-fairness as a political, rather than a comprehensive, conception of justice (which was present in embryo in *A Theory of Justice*, [p. 8]) is either unworkable and internally inconsistent or far less of a change than it is claimed to be. Since I am unconvinced that the aspect of stability that Rawls aims to solve by separating out the political as the focus of justice is really a problem, the unworkability of his solution in no way detracts, in my view, from the main arguments of *A*

Theory of Justice. But the newer ideas seem to lead into a maze of real difficulties.

First, the dichotomy Rawls draws between the political and the nonpolitical (which he applies to his conception of the person, as well as to institutions), though useful within his philosophical argument, seems impracticable. For one thing, it is undeniable that some of Rawls's "nonpolitical" institutions—notably, families and churches—"in fundamental ways shape citizens' character and aims" (p. 68)—which is one of the defining characteristics of the basic structure. For another, such a conceptual "splitting" of persons surely works to provide the stability that Rawls aims at only if real human beings can think of themselves this way. Since, as Rawls acknowledges, those spheres he regards as not political constitute, by far, the larger part of people's daily lives, this is difficult to imagine. For example, if, in the just society, families and churches are not required to be just but can be organized hierarchically (with this ordering justified by "nature," say, or divine law), how is it possible for those who spend a far greater (as well as more intense) part of their lives in these institutions than in any "political" activity to think of themselves as free and equal citizens, as Rawls requires them to in the political realm?

Related to this is the problem of how persons living in such settings are to develop a "sense of justice"—or what Rawls now calls the "political virtues." Although he regards these qualities as another crucial part of what renders the just society stable, *Political Liberalism* makes scant reference to the careful and largely persuasive account of the development of a sense of justice contained in chapter 8 of *A Theory of Justice*. Not only is it reduced to two paragraphs, but families and associations—which formerly played pivotal roles in it—are missing. Perhaps Rawls means us to carry over his theory of moral development from *A Theory of Justice*. If so, a significant problem still remains. There, in order to be satisfactory loci for moral development, families and associations were assumed and recognized to be *just*. In the new account, the political/nonpolitical dichotomy seems to preclude this. If this interpretation of *Political Liberalism* is accurate, Rawls, by addressing one part of the problem of stability, has exacerbated the other.

But *is* this interpretation accurate? Now I come to a second problem with the dichotomy so central to *Political Liberalism*, namely, that it is not consistently held to. Rawls says that the theory of justice is limited to the domain of the political and that other, more comprehensive conceptions will regulate the other aspects of people's lives. Early in the book, he several times says that while there are many reasonable comprehensive doctrines, there will always be some unreasonable ones, which need to be "contained" (pp. xvi–xvii, 39). Subsequently, however, he argues that only reasonable ones will be "allowed" or "permitted." His examples of those that may be discouraged or even excluded altogether include doctrines and associated ways of life "in direct conflict with the principles of justice," such as those "requiring the repression or degradation of certain persons on, say, racial, or ethnic, or perfectionist grounds" (pp. 195–96).

This involves several significant constraints. It certainly means that certain ways of life (e.g., slavery, serfdom, or a religion enforced by the state) cannot be legislated even if a majority fervently believes in them. In this sense, they are "off the political agenda" (pp. 151–52, 161). It also means that persons cannot choose to live such lives, in a just society: "only permissible conceptions (those that respect the principles of justice) can be *pursued*" (p. 193, emphasis mine; see also p. 176, n. 2). It seems clear, then, that Rawls's just society would not permit the practice of unreasonable conceptions of the good—those that violate the principles of justice. But how is one to reconcile these constraints with the central claim of the book, namely, that justice as fairness is now a strictly political theory of justice? It seems that though he frequently denies it, he *is* arguing that persons in the just society should order their whole lives—not only the political aspects of them—in accordance with justice.

If this interpretation is correct, then *Political Liberalism* is far less of a departure from *A Theory of Justice* than Rawls claims it to be. I, for one, find its arguments and conclusions, thus interpreted, more convincing and workable than those which Rawls sets out to be making. Also, if justice is to apply to the whole of life, not just political life, then both of the problems of stability are resolved. Citizens of the just society are unlikely to have trouble endorsing and living by the principles of justice if their various conceptions of the good are fully in accordance with them. And their growing up within just institutions—families, religions, and schools, as well as the constitutional framework of their society—should provide that environment needed for the development of a sense of justice in its citizens without which a just society cannot survive.

Stanford University SUSAN MOLLER OKIN

Law's Violence. Edited by Austin Sarat and Thomas R. Kearns. Ann Arbor: University of Michigan Press, 1992. 261p. $34.50.

Punishment: Theory and Practice. By Mark Tunick. Berkeley: University of California Press, 1992. 211p. $35.00.

Law professor Robert Cover reintroduced the community of legal academics to the Weberian proposition that law should be understood as the institution embodying the state's monopoly of legitimate violence. The essays in *Law's Violence* explore how scholars in various disciplines can elaborate Weber's (and Cover's) insights, in this postmodern time.

Elaine Scarry, whose specialty is literary criticism, provides a case study of declarations of war to show how legal texts operate as performative utterances and are thereby connected to the legitimacy of the exercise of violence. Her literary sensibility allows her to discover nuances in expression that characterize declarations of war. These expressive forms demonstrate, Scarry argues, that the Constitution requires careful deliberation before the United States enters into war. Scarry criticizes modern exercises in warmaking for failing to comply with the Constitution's requirements for declarations of war. She concludes that the first use of nuclear weapons at the president's sole initiative violates the Constitution. Her intriguing analysis is flawed only by a failure to consider whether the violent activities in which modern armies engage ought to be described as "war" within the meaning of the Constitution.

In a more mundane analysis, Judge Patricia Wald argues that the legal system ought to confront the implications of its violence more directly—in criminal sentencing, prison conditions, and family law. She urges that the legal system incorporate mechanisms by which this violence can be brought to the attention of decisionmakers and the public. For example, she criticizes sentencing guidelines for obscuring the real impact of criminal sentencing on offenders' lives.

Three other essays examine different dimensions of the state's claim to a monopoly of legitimate violence. Relying on anthropological studies of the Ilongots of the Philippines and the Kaluli of Papua New Guinea, Carol Greenhouse argues that the "textual" violence of law is continuous with—not sharply distinguished from—private practices of violent dispute resolution. She offers a reading of the Supreme Court's decision in *Perry* v. *Louisiana* (1990) (which dealt with the constitutionality of administering antipsychotic drugs to a defendant sentenced to death, to "cure" his delusions so that the state could execute him) showing how violence is embedded in legal texts. By bringing law's violence to the surface, Greenhouse's reading shows that legal interpretation rests on the existence of a community of value with which capital punishment may be radically inconsistent.

Douglas Hay provides a history of capital punishment and incarceration, particularly emphasizing the distinction between the "high law" administered in state trials and other celebrated cases and the "low law" administered to more routine felons. The distinction between "high law" and "low law" provides Hay with the opportunity for a particularly interesting discussion of the Archer Shee case, the inspiration for Terrance Rattigan's play *The Winslow Boy*. Hay suggests that the different types of law illustrate the legal pluralism that interested Cover and that the state uses the veil of legitimacy created by "high law" to justify its direct application of violence through "low law."

Although Robert Weisberg acknowledges that his essay has only an "oblique" connection to the book's larger themes (p. 175), his emphasis on private violence as an effort to enforce lawlike norms also pursues the theme of legal pluralism.

Coeditors Austin Sarat and Thomas Kearns provide a reading of Cover's work that attempts to reconcile the anarchist implications of Cover's perspective (implications that Cover acknowledged) with Cover's endorsement of at least some forms of state-administered law. Their effort, while interesting, is unsatisfying—more from the gnomic nature of Cover's work than from their failure to reconcile incompatible themes. Cover's emphasis on law's violence was important in forcing centrists in the legal academy to acknowledge that their obsession with legitimacy obscured the reality of law at its point of application. It is unclear whether Cover's work, in fact, substantially deepened the Weberian, or any other, legacy. The attention his work has attracted may result more from his institutional position at Yale Law School, his personality, and his premature death than from its substance.

Mark Tunick's *Punishment* is an elementary exposition of some philosophical issues connected to the practice of punishment. Tunick introduces readers to a standard distinction between external (particularly, radical) and internal criticisms of practices. While internal criticisms attempt to make the best sense of practices to those engaged in them, so that people can feel "at home" in the practices (perhaps as modified to polish some rough edges), radical criticisms are aimed at making people feel uncomfortable with the practices (pp. 62–63). He then outlines familiar utilitarian and retributivist accounts of punishment. Tunick argues that retributivism "gives a better, more complete account of why we punish" (p. 15). His most interesting discussion uses the retributive principle as the basis for internal criticisms of some aspects of our actual practice of punishment, such as plea bargaining.

On the whole, however, Tunick's treatment is quite superficial. Much turns on the extent to which readers agree with Tunick's largely undefended intuitions about what amounts to a satisfactory justification of our practices. His well-written book might be helpful to those seeking an introduction to the philosophical issues associated with punishment, but students seeking deep insights will have to look elsewhere.

Georgetown University MARK TUSHNET

AMERICAN POLITICS

Senate Elections. By Alan I. Abramowitz and Jeffrey A. Segal. Ann Arbor: University of Michigan Press. 262p. $45.00 cloth, $16.95 paper.

This is a curious book. It is about elections but not primarily about voters. It is published by a university press and has some findings that will be of interest to the scholarly community, yet its primary audience seems to be advanced undergraduates. It acknowledges that we know a great deal about Congressional elections but rarely cites other works (particularly those on Senate contests).

Alan I. Abramowitz and Jeffrey A. Segal pull together much of their important work on Senate elections in this volume. They show that Senate challengers are better known than their House counterparts, that Senate incumbents are not as well liked as their House counterparts, that challenger spending is the best predictor of vote shares in Senate elections from 1974 to 1986, and that incumbent spending matters, too. Issues matter in Senate election; so does partisanship. One of the most important insights of the book is that senators face less hospitable partisan environments than do representatives, which accounts for much of the lower reelection rate for members of the upper chamber. Yet the volume has no overarching framework, no grand theme that one can hail or attack. It is not like Jacobson and Kernell's *Strategy and Choice in Congressional Elections* (1981). Nor do the authors offer an uncontroversial but distinctly novel argument, such as Westlye's *Senate Elections and Campaign Intensity* (1991).

Abramowitz and Segal are at their best when presenting models of vote shares in primary and general elections (chaps. 3–4). Candidate ideology, scandals and other controversies, candidate spending, and challenger quality determine vote shares in primaries. The authors develop a 16-variable model for incumbent general election contests: only presidential popularity fails to be significant at $p < .05$. Most of the variables are familiar to students of congressional elections. The novel variable is aggregate perceptions of party competence.

These models still have their limitations. Abramowitz and Segal argue that aggregate economic factors play no direct role in vote shares but, rather, affect party competence. They offer a separate regression for party competence but do not attempt the more appropriate two-stage least squares estimation. They hail their model's predictive success (off by two seats in 1986 and 1990 and three in 1988). If the base is the observed losses, rather than the total number of seats, the predictions appear far less accurate. They sometimes emphasize standardized regression coefficients when unstandardized measures are more appropriate and judge the relative impact of clusters of variables by increments in R-squared—inappropriate whenever there is any correlation among the independent variables.

Almost every time they use regression analysis, the authors explain how to interpret the betas. This, together with numerous case studies, suggests that the real audience is advanced undergraduates. Abramowitz and Segal offer case studies of the six senators who lost primaries from 1974 to 1990 in chapter 3. Two chapters are devoted to case studies on incumbents who won (Alan Cranston and Alfonse Damato in 1986) and those who lost (Paula Hawkins and James Abdnor in the same year). Each of these will appeal greatly to students; but collectively (or individually, for that matter), they do not add up to a scholarly contribution. The authors cite ABC News exit polls on each of the latter four races but do not provide their own multivariate analyses of these data. Nor do they offer an individual-level model of Senate voting in the one chapter (chap. 2) that employs National Election Study data. Despite the promise early on, the special Senate studies of 1988 and 1990 are barely used. Only one table (Table 2-13, voter familiarity with Senate candidates by size of state) employs the 1988 Senate data.

Students will appreciate these case studies, as they will appreciate being guided through regression time and time again. They will also enjoy the history of Senate elections—how we moved from appointment by state legislatures to direct election and primaries—in chapter 1. The discussion of money in Senate elections is also lucid and interesting. The authors' analysis of campaign spending reform—one of Senator David Boren's proposals—finds that challengers and open-seat candidates would gain the most from spending caps. Students, instructors, and activists will find much to argue about in this unconventional conclusion.

I don't mean to be too negative about this book. Abramowitz and Segal deserve credit for putting Senate elections in perspective. Their aggregate models are important contributions. Their focus on how the partisan balance of constituencies affects outcomes is a distinctly novel contribution. The case studies flesh out their argument nicely and will keep students interested. The writing is far above the usual standards for political science, though not at the level of the *Economist* or the *Wall Street Journal*. This book deserves to find a niche, for there is none like it.

University of Maryland, College Park ERIC M. USLANER

The Budget-maximizing Bureaucrat: Appraisals and Evidence. Edited by André Blais and Stéphane Dion. Pittsburgh: University of Pittsburgh Press, 1991. 366p. $39.95.

In this volume, Blais and Dion assemble eleven essays by notable authors appraising past and present research that tests William Niskanen's influential theory of budget interactions between bureaucracies and legislatures. The essays are revised versions of invited papers delivered at a conference held at the University of Montreal in April 1989. The various essays review existing literature, suggest interesting hypotheses, and also present some original empirical research on bureaucracy and budgets.

The first chapter, authored by William Niskanen, reassesses the positive theory of bureaucratic dominance and inefficiency depicted in his earlier work. In a refreshing burst of intellectual candor, Niskanen admits that the assumptions built into his original model were wrong. Specifically, he notes that the assumptions of budget maximization and purely self-interested bureaucrats are too restrictive for a general model. He also implies in his discussion that Downs was correct in depicting a range of bureaucratic motivations—including zealotry, which leads to preferences for higher outputs, rather than higher salaries. His fix for the problem is to revise the original model to assume that bureaucracies maximize discretionary budgets (as in his 1975 article). He also suggests that the earlier work be considered as just a special case of diverse bureaucratic behaviors, including another permutation in which bureaucracies actually maximize efficiency for the purpose of increased outputs. Of course, this is a stunning revelation coming from an individual who provided much of the intellectual foundation for the mindless bureau-bashing of the 1980s.

What is not so refreshing is that Niskanen downplays (with no empirical evidence and for no apparent reason) the idea that efficiency or output maximization, rather than budget maximization, is a common phenomenon in bureaucracy. He also asserts, contingent on the obviously incorrect assumption of a passive sponsor, that efficiency-maximizing bureaucracies will always have outputs that are too high. He acknowledges the important contribution of Miller and Moe (APSR, 1983) in showing that outcomes are altered when the agency is faced with an active sponsor. However, he waffles on the implications of active sponsors for bureaucratic budgets, stating "I have not worked out the full implications of this assumption for the budget-output outcomes of the bargaining process" (p. 23). After more than 20 years, one is led to wonder how long it will take him to work out the implications or whether he has paid any attention to the work of excellent scholars in this area who have examined the implications of active sponsors (e.g., Bendor and Moe [APSR, 1985]; Bendor, Taylor, and Van Gaalen [APSR, 1985]; Ferejohn and Krehbiel [AJPS, 1987]; Calvert, McCubbins, and Weingast [AJPS, 1989]; Banks [AJPS, 1989]). In any event, the Niskanen essay is far too kind in its assessment of the worth of the unaltered model for future research.

Many of the other essays in the volume are more critical of the theory but certainly not acrid in their assessments. The chapter by D. Roderick Kiewiet finds the most support for Niskanen's theory. Kiewiet discusses past research that is consistent with key predictions of the theory but coolly concludes that the model "has fared at least as well as any other model in political economy of comparable generality" (p. 166). In chapter 9, Donald Blake reports that public-sector workers have more left-wing—and therefore prospending—views than those in the private sector. Consistently, André Blais, Donald Blake, and Stéphane Dion provide qualified evidence in chapter 8 that bureaucrats are also more likely to support parties of the Left. Both findings are consistent with the Niskanen oversupply thesis, but this is very indirect evidence of actual outcomes. These authors also find that the cleavage between bureaucrats and the public is much smaller than public choice theorists would like to contend. This suggests that the explanatory ability of the Niskanen model is weak. In chapter 9, Jean-Michel Cousineau and Anne-Marie Girard report that public-sector unions are less influential than private-sector counterparts in obtaining benefits for bureaucrats.

Three chapters test the validity of key assumptions of the Niskanen theory. In chapter 2, Robert Young presents evidence showing that bureaucrats do not receive extraordinary benefits when they are situated in bureaucracies with rapidly growing budgets. Referencing past empirical research and survey evidence, he concludes that there is little support for the central Niskanen hypothesis that individual bureaucrats benefit from budget growth. Nevertheless, Laurence Lynn argues in chapter 3 that there is evidence to show that bureaucrats do, indeed, seek larger discretionary budgets. Still, Lynn concludes that the Niskanen model is so parsimonious, general, and difficult to operationalize empirically that it is not very useful as a research tool.

In chapter 4, Colin Campbell and Donald Naulls present what I think is the strongest evidence against the Niskanen model. They use cross-national survey data to evaluate the core Niskanen assumption that personal utility-maximizers in bureaucracies are also budget-maximizers. The data they present are persuasive in showing that just the opposite is true: bureaucrats whose career ambitions are tied to their own personal utility are also those who are predisposed toward budget minimization, not maximization. Thus, when the unit of analysis is the individual bureaucrat, the theory breaks down in predicting bureaucratic behavior.

Several chapters employ a comparative cross-national perspective in evaluating the model. In chapter 5, Peter Aucoin observes that Niskanen's theory has been politically influential, producing recent budgetary restraint in various countries, including the United States, United Kingdom, Canada, and Australia. Yet the theory predicts that bureaucracies should dominate the budgetary restraint process with their superior information and knowledge of production costs. Such efforts at restraint should, therefore, be ineffective. Aucoin argues that such efforts have, indeed, been ineffective but largely because of the efforts of outside actors like client groups and entrepreneurs, not bureaucracies. Bureaucracies, he claims, have been only marginally influential in retarding budgetary restraint processes. Consistently, in chapter 7, Andrew Dunshire marshals evidence from the United Kingdom suggesting that bureaucratic behavior during times of budget restraint conformed more to a "public-service-oriented" model than to the Niskanen "self-regarding" model. Likewise, in chapter 11, B. Guy Peters presents evidence from 14 Western European countries showing that between 1950 and 1987, European bureaucracies did not manipulate government in such a way as to maximize their own budgets and improve the positions of bureaucrats.

In summary, this volume contains essays which appraise the state of current research on William Niskanen's budget-maximizing bureaucrat. It should be interesting reading for anyone doing empirical research on or formal modeling of bureaucratic budget behavior. It provides a good survey of the literature and of the potential issues to be addressed in testing or formulating future models. However, it is of little interest to those focusing more on the legislative side: no single essay deals with legislative behavior. My appraisal of the appraisal is, then, that the Niskanen model has neither been validated nor invalidated by the work presented in this volume. Readers looking for good background material should look here, but those looking for definitive answers should look elsewhere.

Texas A&M University B. DAN WOOD

Who Benefits from the Nonprofit Sector? Edited by Charles T. Clotfelter. Chicago: University of Chicago Press, 1992. 285p. $22.95.

Inventing the Nonprofit Sector and Other Essays on Philanthropy, Voluntarism, and Nonprofit Organizations. By Peter Dobkin Hall. Baltimore: Johns Hopkins University Press, 1992. 349p. $36.95.

Nonprofits for Hire: The Welfare State in the Age of Contracting. By Steven Rathgeb Smith and Michael Lipsky. Cambridge: Harvard University Press, 1993. 292p. $35.00.

The choice between politics and markets is a recurring concern of political scientists. In recent years, that choice has become more complicated, due to the emergence of a strong third sector—the nonprofit sector. There are now over 1.2 million nonprofits in the United States, and nonprofits account for 10% of our nation's workforce. In belated recognition of these facts, scholars from various disciplines are beginning to take nonprofits seriously.

Three recent books illustrate the range of relevant questions. Hall traces the social, political, and economic history of nonprofits. Clotfelter's contributors attempt to determine which income classes benefit most from nonprofits. Smith and Lipsky assess the growing role of nonprofits in delivering public services through government contracts.

Inventing the Nonprofit Sector, by Peter D. Hall, is a collection of loosely integrated essays on the evolution of nonprofits. As a social historian, Hall is attentive to political, cultural, and intellectual crosscurrents. As he sees it, the paths taken by philanthropy and voluntarism parallel the paths taken by American society at large. The advantage of this approach is that virtually no cultural fragment is irrelevant to his story; the disadvantage is that the storyline sometimes evaporates because the fragments receive so much attention.

Hall's essays were culled from prior publications, without scrupulous editing. As a result, the manuscript has a certain cut-and-paste quality. The last three chapters are particularly weak. In chapter 7, Hall berates stingy administrators and unappreciative colleagues, who undervalue research on nonprofits; in chapter 8, he tells us more about recent intramural debates than we really need to know; and chapter 9 reads like an after-dinner speech.

The manuscript suffers from typographical errors and repetition. For example, quotes by Alexis de Tocqueville and Richard Ely appear twice in the same form. More seriously, the author sometimes ignores important points that he himself made in a previous chapter. For example, in chapter 2, Hall argues that "managerial professionalization has tended to reduce the policy role of governing boards in favor of staff control." This suggests that we need to know more about staff members, their recruitment, and their standard operating procedures. Yet chapter 4 focuses at great length on boards of trustees, whose power has supposedly declined. If boards of trustees have been eclipsed by professional staffs, why devote one fourth of the book to "cultures of trusteeship"?

Normatively, the author also seems to lose his bearings along the way. In chapter 2, he hints at a close positive connection between "small groups of large donors" and "pluralism, innovation, and excellence." Yet in chapter 4, he berates those who wallow in "nostalgia for the 'good old days' of WASP hegemony." At times, it is difficult to determine whether Hall is a democrat in aristocratic clothing or vice versa.

Inventing the Nonprofit Sector is not without virtue. Hall argues persuasively that the nonprofit sector is, in reality, a dependent sector, not an independent sector (as is sometimes claimed). Moreover, he offers historical evidence that this dependent relationship is not new. Even Harvard University, it turns out, originated as a quasi-public entity, funded primarily by the state and governed in good measure by public officials. Close students of nonprofits will find other interesting tidbits buried throughout the book. But teachers are likely to conclude that the book is too discursive for classroom use.

In contrast to Hall, Clotfelter's contributors focus much more sharply on a single question: Who benefits from the nonprofit sector? Several economists and one political scientist attempt to answer that question in separate chapters on health services, education, religious organizations, social services, arts and culture, and foundations. An introductory essay by the editor sets the stage for this exercise, and tart commentaries by three prominent economists conclude the volume.

Who Benefits from the Nonprofit Sector? is uneven in quality, largely because data availability varies from sector to sector. David Salkever and Richard Frank, who examine health services, are able to make explicit comparisons between nonprofits and other institutions. They find, for example, that nonprofits are typically less redistributive than public institutions, such as public hospitals, public nursing homes, public drug treatment facilities, and public facilities for the mentally ill. Comparisons between nonprofits and for-profits are more difficult to summarize. In mental health and drug treatment, nonprofits are more likely than for-profits to serve the poor. Among nursing homes, however, nonprofits are actually less likely to care for Medicaid patients.

Lester Salamon, who discusses social services, enjoys access to excellent data on how the percentage of poor clients varies across different types of social service organizations. He finds that employment and training and legal rights organizations are most likely to serve the poor, while agencies specializing in education and research are least likely to serve the poor. More broadly, he concludes that 53% of social service agencies serve few or no low-income clients. These are sobering findings, indeed.

Jeff Biddle, who examines religious organizations, could only pray for such robust data. Lacking hard evidence on how churches spend their money, he is forced to make arbitrary estimates on how much spending is allocated to mutual benefit purposes (70%) and how much to philanthropy (30%). Robert Margo, who examines foundations, and Dick Netzer, who examines the arts, also struggle with data limitations.

Less obviously, none of the contributors is really able to answer the central question that animated this volume. As Henry Aaron points out in a particularly trenchant commentary, "One cannot infer that if the nonprofit sector were somehow transmuted into for-profit enterprises or into publicly owned facilities, the characteristics of the groups they served would suddenly resemble those now served by for-profit or publicly owned facilities"; or, to put it a bit differently, the counterfactual is not easily specified for services provided by the nonprofit sector. Another limitation, noted by Estelle James, is that none of the contributors is able to estimate net benefits. Instead, they are forced to assume that net benefits are proportional to usage and that the distribution of net benefits across income classes is the same as the distribution of usage.

Perhaps because of these difficulties, Clotfelter and his contributors are unusually cautious in drawing inferences and making recommendations. Indeed, the book does not move beyond empirical evidence to public policy design. Should governments subsidize nonprofits less? Should governments do a better job of linking subsidies to redistributive policy outputs? Where should we go from here? Unfortunately, they do not attempt to answer these questions.

The book, however, is valuable in two respects. First, it represents a commendable effort to move beyond questions of efficiency to questions of equity in empirical studies of nonprofits. The fact that most of the contributors are economists reminds us that many public economists care about redistributive consequences and have attempted to operationalize them. Second, the editor's intellectual honesty yields a book that could be very instructive to students in research methods courses. Trade-offs between important questions and definitive answers are aptly illustrated throughout this book, and the concluding commentaries are unusually revealing and incisive.

In contrast to Clotfelter's contributors, who draw few conclusions from several complex data sets, Smith and Lipsky present a strong point of view buttressed by numerous examples from mental health, protective services, drug rehabilitation, and other social service fields. They are concerned about the growing role of nonprofit organizations in delivering public services, which they believe has contributed to an "unbalanced reciprocity" between government funding agencies and financially needy nonprofits. They recommend that nonprofits fight more aggressively to preserve their distinctiveness against government encroachments and restrictions.

Smith and Lipsky (both political scientists) develop several intriguing ideas—that we have unwittingly established a "contracting regime" in which nonprofit organizations are agents of the state, that nonprofit staffers are the new "street-level bureaucrats," that nonprofit managers prefer responsiveness to equity, that government regulation of nonprofits promotes standardization and professionalization, that materialistic incentives are displacing purposive incentives, and so on. The authors support these ideas, to varying degrees, by drawing upon observations from 30 nonprofit organizations that deliver social services. They conclude that mutual dependency is a bad bargain for both nonprofits and their government benefactors.

Nonprofits for Hire is an interesting, absorbing, and important book. It is well-written, thoughtful, and provocative. There are nuggets of insight in every chapter, and the chapters are well integrated. The authors use both wide-angle and zoom lenses to good advantage. They also do an excellent job of linking political and administrative processes to policy results. Their book is likely to be a big hit in public policy and public administration courses, at both the graduate and undergraduate levels. It is also likely to generate considerable controversy.

The implicit premise behind *Nonprofits for Hire* is that if nonprofits do not deliver social services, such services will be directly provided by government officials. With this premise in mind, the authors compare nonprofits and governments throughout their book (especially in chap. 6). Yet one could argue that in American society, the real counterfactual is not a European-style welfare state but, rather, a "contracting regime" in which contracts are awarded to for-profit firms.

For-profit firms receive scant attention in this book, which makes it difficult to determine whether the key changes in recent years have been in the government's willingness to contract out or in the government's selection of nonprofit firms to do the contracted work. It would also be useful to have a clearer sense of how patterns vary throughout the United States. Of Smith and Lipsky's 30 nonprofits, 25 come from Massachusetts and all come from the Northeast. In other states and regions, the pathologies of contracting may be either more or less severe.

The authors' most controversial assertions are not empirical but normative. While acknowledging the need for accountability in government contracting, they warn that government regulations rob the nonprofit sector of its originality and threaten to substitute bureaucratic officiousness for empathy, sensitivity, and informality; that is, of course, one way to formulate the dilemmas of dependency. But a different formulation would stress the government's right to insist upon quality, equity, and fairness (not to mention efficiency).

If government regulations result in better-trained child-care providers, is that undesirable? If government funds induce mental health facilities to handle tougher cases, is that undesirable? If government funds induce nonprofits to focus more sharply on the most vulnerable members of society, is that undesirable? Left to their own devices, nonprofit organizations will not rush to care for the neediest of citizens. Indeed, that is one of the lessons of the Clotfelter book. Thus, we need to decide whether the preservation of a robust and quirky nonprofit sector is ultimately more important than the targeting of social services to those who need them the most. It is to Smith and Lipsky's credit that their book generates such questions. Few readers will agree with every word of this book, but few will put it down without reconsidering the role that nonprofit organizations play in our society.

Despite some imperfections, the simultaneous publication of three new books on nonprofits is a welcome development. The third sector is indeed a dependent sector, with important implications for government programs and their clienteles. Increasingly, one cannot understand public bureaucracies without understanding the new street-level bureaucrats whose incentives tug them in different directions and whose norms, increasingly, are up for grabs.

Georgetown University WILLIAM T. GORMLEY, JR.

American Chameleon: Individualism in Trans-National Context. Edited by Richard O. Curry and Lawrence B. Goodheart. Kent: Kent State University Press, 1991. 276p. $35.00 cloth, $17.50 paper.

Is America Different? A New Look at American Exceptionalism. Edited by Byron E. Shafer. New York: Oxford University Press, 1991. 266p. $59.95.

In his contribution to *Is America Different?* Daniel Bell quotes the psychologist Henry Murry: "everyone is in some sense the same as everybody else; everyone is in some sense the same as somebody else; and everyone is in some sense the same as nobody else." This is the wisdom of universals, which tells us everything and nothing. It applies, for example, to both books under review, but it does not speak to the fact that one is mediocre and the other outstanding; it applies to Bell's essay in a way that conceals the essay's extraordinary richness and depth; and it applies to the United States without accounting for the crucial and abiding problem of difference. That problem is generally identified as "exceptionalism," a term that seems to have originated in an old radical debate (why has socialism failed in the United States?), but that reaches forward to the immigrant dream of opportunity ("only in America") and back to the quasi-religious rhetoric ("errand," "manifest destiny," "city on a hill") through which the United States gradually usurped for itself the symbolic meaning of "America." Is there something distinctive about that process? Is there some special configuration of political and symbolic factors that makes for a different *American* identity, and makes the difference so significant that it may be said to subsume or even reshape the manifold similarities between the United States and other modern nations?

The question is particularly pertinent nowadays because the very concept of an American identity has come under attack. Multi-culturalists deny the claims of Americanness; social historians have turned consensus into a term of derision; literary critics speak of American literature, if at all, in the plural, as a diversity of traditions, audiences, even languages. The national melting-pot has become a post-colonial patchwork quilt. Commonplace contrasts between the Old World and the New have been "subverted" through such meta-geographical categories as "empire" and "patriarchy." And yet, outside the groves of academe, the language of American exceptionalism continues to resonate everywhere, here and abroad. So *is* America different? To judge from the essays gathered in these two books, the answer is Yes.

Of course there are qualifications. The editors of *American Chameleon*, Richard O. Curry and Lawrence B. Goodheart, highlight the "trans-national context" in their subtitle and, in their concluding essay, they outline a contemporary "confusion of voices" (p. 188). A number of essayists in this volume dwell on the "republican ethos" (as distinct from liberalism) that characterized the first decades of nationhood. And several essays in *Is America Different?* speak of difference more or less nostalgically, as a mixed blessing that has passed—the exceptionalism of a wholly civil society (Bell), of free land (Peter Temin), and of the creative tension between individualism and egalitarianism (Aaron Wildavsky). These qualifications, however, turn out to be a matter of degree, not of kind. Civil association (as distinct from the Hegelian state) remains a prominent characteristic of this society; the energies inspired by the promise of free land have been successfully rechannelled into other areas of free enterprise; the culture continues to be nourished by the reciprocities between individualism and egalitarianism; and, in face of the overwhelming testimony in *American Chameleon* to what Curry and Goodheart call the "pervasive influence" of individualism in American history (p. 1), the arguments about an antagonistic republicanism sound antiquarian, not to say strained. According to James Henretta, "Federalists and their Jeffersonian opponents still lived in a world that, in social composition and political ideology, was more republican than liberal" (p. 91); but "more" signifies less and less in proportion to what we learn (in both books) about the social importance of federalism in liberal America and the political ideology behind Jefferson's expansionist policies and plans for industrialization. As Byron E. Shafer points out in his concluding overview, the "major themes" linking the various essays in *Is America Different?*—"populism and individualism, market-making and democratization" (p. 223)—are still dominant in one form or another through all areas of American life and thought.

One of these themes, individualism, is the focus of *American Chameleon*. More than a third of the book (five of its fifteen essays) was written by the editors—a two-man project clearly, which nontheless spans several generations of scholarship in social and intellectual history, from Yehoshua Arieli, whose 1964 study of individualism is a staple reference in the field, to Lawrence Goodheart, who did his doctorate under Curry's direction. The essays are generally competent and informative, but without surprise or special distinction. They range from the careful but tiresome (Curry and Karl E. Valois on the emergence of American individualism) to the impassioned but predictable (Linda K. Kerber on the disadvantages to women of the discourse of self-reliance). Robert M. Calhoun offers a solid overview of religion and individualism in early America; Loren Schweniger succinctly rehearses the intricate links between race, freedom, and individualism. The book centers on the nineteenth century but I believe it will prove most useful for its chronological coverage, extending as it does to colonial antecedents as well as twentieth century developments. Its value lies not in "trans-national context" but in the methodical way it elaborates upon the well-known assertion of James Bryce (1888) that individualism has "been deemed by Americans not only their choicest, but peculiar and exclusive possession" (p. 1).

Lord Bryce was simplifying. Individualism is only one of several such prized, peculiar, and at times mutually conflicting possessions, and it has been the genius of the American Way to hold these together, or to hold together through these, despite an extraordinarily changeful and violent history. This is the import of Shafer's volume, a collection of (revised and expanded) talks delivered at a conference at Nuffield College, Oxford. It must have been a wonderfully stimulating event. Each of the eight essays is provocative, elegant, and learned (a rare combination). They are also refreshingly different from each other in outlook and method. Two essays depend heavily on statistics: Richard Rose's authoritative account of public policy in the United States vis-a-vis other OECD nations and Americans' own perceptions of government; and Andrew W. Greeley's feisty, tough-minded report on religious commitment in our time: contrary to the lamentations over decline (a cultural reflex from Cotton Mather to Robert Bellah), Americans, Greeley shows, have remained remarkably pluralistic and remarkably devout (now as fifty years ago 95% believe in God, 77% in Jesus, and 72% in life after death, with another 20% unsure). These findings supplement, rather than clash with, the secular emphases of other essays—for example, Temin's sweeping economic analysis of the reciprocities between federalism and free land in the nineteenth century; an analysis that reaches beyond its immediate subject to offer brilliant insights into the entire "American System": Yankee culture, industrialization, tariffs, etc., and perhaps most intriguingly, the importance of arms production to the history of American commerce and manufacture.

Four essays in this collection are genuinely major contributions to our understanding of the United States, well beyond issues of exceptionalism. If Martin Lipset's keynote "reaffirmation" of the American difference is in some ways a summation of his previous comparisons of Canada and the U.S., it is also a beautifully-conceived, beautifully-executed survey of the whole question of "Americanism," in all its tangled complexities, negative and positive, from ideological and political "variables" to urban crime, divorce rates, and the excesses of chauvinism (p. 1, 8). Bell's essay on the passing (with FDR's New Deal) of what he calls a "complete civil society, perhaps the only one in the history of political societies" (p. 57) is in effect a profound meditation, at once philosophical and culturally comparatist, on the nature of government, modernism, and the state. It should become a classic for American studies at large. Aaron Wildavsky and Martin Trow write respectively as theorist and as historian. That's somewhat deceptive, since Wildavsky's essay is also historical and Trow's is also speculative; but the first depends on an abstract model and the second on the story of American education. Wildavsky's model builds on the multiple tensions (social, political, economic) between individualism and egalitarianism; he might well have applied his theory to other areas, including the aesthetic. His generalities (e.g., on how American egalitarians tend to ally with individualism) provide a key to connections between literary form and political structure. His definitions of difference provide some of the clearest formulations I know of "Americanness." One example must suffice: "America is about possibilities, not certainties. You can (but you don't have to) get rich. America is about promises. Here class divisions do exist but they don't have to matter. . . . Exceptionalism is the doctrine that justifies the promise" (p. 128).

Perhaps the single most persuasive practical instance of that process of justification comes in Trow's long essay on American higher education. It is a stunning demonstration of quantitative research in the service (rather than at the expense) of ideas, specialized historicism that opens up large cultural connections. There is an unspoken paradox at the center of this analysis: how did our market-based, leveling, mass-production society develop the greatest system of elite education in the world, probably of all time. Trow explains this with such authority (of fact and of reason) that it seems no paradox at all, but rather the logical product of a peculiar set of economic, cultural, and social factors. Higher education here becomes the framework for exploring a spectrum of historical continuities and institutional structures, and in the course of exploration Trow illuminates not only the contours of the immediate environment but the surprising relations between apparently disparate phenomena in the surrounding world, both in their own right and in their differences from (and similarities to) adjacent worlds. This essay should be mandatory reading for all Americanists.

It would be ungrateful to complain about absences in a book of such outstanding merits. Indeed, the space allotted to this review has not allowed for an adequate description of it merits, among these its continuous and continuously stimulating comparatist emphasis. (Happily, that absence is more than adequately filled by Shafer's balanced and perceptive summary essay). Let me say instead that *Is America Different?* is a standing invitation to other, parallel conferences in the humanities, arts, and sciences. Both these books confirm the abiding challenge of the *problem* of "Americanness"; Shafer's volume sets an intellectual and scholarly standard for discussion and debate.

Harvard University SACVAN BERCOVITCH

Left Coast City: Progressive Politics in San Francisco, 1975–1991. By Richard Edward DeLeon. Lawrence: University of Kansas Press, 1992. 256p. $35.00 cloth, $14.95 paper.

Politics in Black and White: Race and Power in Los Angeles. By Raphael J. Sonenshein. Princeton: Princeton University Press, 1993. 301p. $29.95.

Richard DeLeon and Raphael Sonenshein offer important and needed studies of California's politically vanguard big cities. In the early 1970s, powerful and long-lived progressive coalitions emerged in San Francisco and Los Angeles. In the Bay Area, a burgeoning slow-growth movement helped elect two mayors—George Moscone and Art Agnos—and secured voter approval for the most restrictive growth-control legislation of any big city in the nation. In Los Angeles, a biracial coalition of African Americans and white liberals headed by Mayor Tom Bradley ruled for twenty years.

DeLeon and Sonenshein are the Boswells of California's twenty-year experiment with big-city progressivism. That experiment now calls for serious examination, since it appears to have been ended by the recent election in both cities of right-of-center mayors.

DeLeon's engagingly written study traces the rise and fall of San Francisco's diverse progressive coalition composed of the "three lefts": minority and working-class liberals, Anglo middle-class environmentalists, and neighborhood populists. This essentially slow-growth movement—what DeLeon terms an "antiregime," or blocking coalition—rose in reaction to the city's powerful 1960s-era progrowth regime dedicated to "Manhattanizing" downtown San Francisco and to transforming the city into a major Pacific Rim trading and financial center. Progressivism's greatest successes were scored in the mid-1980s with the passage of stringent growth controls and the election of Art Agnos as mayor.

As DeLeon observes, the progressive movement quickly faltered as it faced the difficult task of transforming a diverse electoral coalition into a governing coalition. The author places most of the blame on the leadership failures of Mayor Agnos, a "liberal deal maker" in progressive clothing. Agnos's championing of large-scale development projects angered environmentalists and neighborhood activists, contributing to his losing his 1991 reelection bid.

There is a competing structural explanation for progressivism's downfall that deserves equal consideration—the existence of deep fissures within the coalition. The city's affluent Anglo environmentalists, for example, displayed little sympathy for the bread-and-butter job concerns of minorities and the working class.

Left Coast City has difficulty deciding whether San Francisco's slow-growth coalition is merely a temporary local phenomenon or, instead, represents a permanent transformation, with national implications, of the post-industrial urban political landscape. The weight of the evidence points to the coalition's limited applicability.

Influential members of the powerful business community used the slow-growth movement for their own purpose, namely, to tighten an overbuilt commercial real estate market. Of the city's recent mayors, only progrowth moderate Dianne Feinstein has established national political credentials. Considering the limited success of the city's slow-growth coalition in power, coupled with the recent election of progrowth mayor Frank Jordan, it is hard to view San Francisco as an exportable model for slow-growth politics.

In both West Coast cities, the character of the progressive regime was shaped by the policies of the previous regime. In San Francisco, an environmentally led progressive "antiregime" replaced a liberal progrowth regime that had tried to incorporate the city's minorities. Not so in Los Angeles. As Raphael Sonenshein points out, Southern California's progressive movement, because of the exclusionary policies of the earlier conservative regime, emphasized biracial coalition building and ethnic incorporation, rather than a slow-growth agenda.

In Los Angeles, conservative WASPs had ruled the city throughout the first half of the twentieth century. The WASP-led progrowth regime deliberately kept the growing Jewish, African American, Latino, and Asian American communities outside the citadels of municipal power. As a consequence, Los Angeles' progressives concentrated upon ethnic political incorporation.

Sonenshein's admiring study of Los Angeles' Bradley coalition seeks to demonstrate that the model of biracial "crossover" politics developed by Browning and colleagues in *Protest Is Not Enough* (1984) applies to the biggest West Coast city. The author distinguishes the supposed Western urban pattern of interracial political cooperation from the racially polarized pattern of Eastern and Midwestern cities. In many ways, *Politics in Black and White* can be read as a *cri de coeur* about the rise—and fall—of black—Jewish political relations. Successful black—Jewish cooperation in Los Angeles is sharply contrasted with the disheartening failure in New York City.

The best parts of Sonenshein's study involve the road to power. The author deftly traces the origins and evolution, in the early 1960s, of the biracial coalition in Tom Bradley's councilmanic district, its citywide spread in the late 1960s, and the coalition's probusiness transformation and internal fragmentation following Bradley's 1973 mayoral victory. Sonenshein's historical account of the coalition's early years is enriched by his personal participation in the Bradley administration and in the political campaigns of several Bradley allies.

The second half of *Politics in Black and White* (on the coalition in power and its future) is on weaker ground. The author overstates the progressive policy accomplishments of the Bradley regime. As the author admits, much of the early trickle down to the inner city came from federal grants, not from local sources. Available in the 1970s, intergovernmental assistance withered in the Reaganite 1980s.

At the local level, Bradley had to enlist the support of the powerful downtown business community in order to govern. Business support came at a price. The business establishment demanded low taxes and a commitment to downtown, not inner city, redevelopment. The mayor's embrace of the business agenda (and its promise of campaign contributions and support) severely limited the local governmental resources that could be allocated to minorities.

Sonenshein also overstates the durability of the black—white liberal political alliance. Like its San Francisco counterpart, Los Angeles' liberal coalition split over growth issues. Bradley's progrowth policies alienated white liberal environmentalists, contributing to the coalition's gradual post-1985 disintegration. The 1992 riots further strained relations between blacks and white liberals, opening the door in 1993 for the election of Republican businessman Richard Riordan.

Despite the dissimilar policy agendas of the two West Coast progressive coalitions, these books share much in common. Both authors optimistically conclude that progressive electoral coalitions and policies can work in the big cities, not merely in college towns like Berkeley, Madison, and Burlington. However, a careful reading of the two case studies suggests a more guarded assessment.

Regarding the difficulties of progressive electoral coalition building, growth issues in both cities pitted affluent Anglo environmentalists against minorities and the working class. Even when biracial electoral coalitions of African Americans and white liberals were constructed, they experienced difficulty becoming truly multiethnic partnerships. In San Francisco, the Chinese—American community—the city's largest minority group—serves as a bit player in DeLeon's slow-growth drama. In Los Angeles, the Latino community, nearly 40% of the city's population, plays a distinctly secondary role in Sonenshein's analysis of the Bradley coalition.

As for the problems of transforming progressive electoral coalitions into governing coalitions, both studies underestimate the continuing power of the business community as a dominant player in big-city politics. Business organizations are particularly influential in California cities, with a long tradition of weak political parties.

When faced with business opposition, San Francisco's slow-growth coalition was only capable of producing a handful of paper-thin victories—and numerous defeats—at the ballot box. It is not surprising, therefore, that Mayor Agnos sought accommodation, not conflict, with the business establishment. In Los Angeles, Mayor Bradley could be elected but could not govern without actively embracing the business community's taxation and redevelopment policies. The recent election—orchestrated by the same campaign manager—of probusiness mayors in San Francisco and Los Angeles shows how conservative governing coalitions can be transformed into victorious electoral coalitions even in liberal bastions.

Notwithstanding their overly optimistic assessments of progressive big-city politics, DeLeon and Sonenshein have produced important books. Each offers a valuable contrasting case to the reigning paradigms of contemporary urban politics—progrowth coalitions (as in Atlanta) and racial polarization (as in New York). Each helps redress the regional imbalance in urban scholarship by illuminating the politics of important West Coast cities. One would hope that both authors would continue their urban vigils. Mayors Frank Jordan and Richard Riordan will test the recuperative powers of progressive big-city politics.

University of California, San Diego STEVEN P. ERIE

Chain Reaction: The Impact of Race, Rights, and Taxes on American Politics. By Thomas Byrne Edsall with Mary D. Edsall. New York: Norton, 1991. 343p. $10.95 paper.

Shadows of Race and Class. By Raymond S. Franklin. Minneapolis: University of Minnesota Press, 1991. 189p. $39.95 cloth, $14.95 paper.

The End of Equality. By Mickey Kaus. New York: Basic Books, 1992. 293p. $25.00.

> The persistence of racism in depth and the dawning awareness that Negro demands will necessitate structural changes in society have generated a new phase of white resistance in North and South. Based on the cruel judgment that Negroes have come far enough, there is a strong mood to bring the civil rights movement to a halt or reduce it to a crawl. Negro demands that yesterday evoked admiration and support, today—to many—have become tiresome, unwarranted and a disturbance to the enjoyment of life. Cries of Black Power and riots are not the causes of white resistance, they are the consequences of it.—Martin Luther King

The fires of Los Angeles in the spring of 1992 temporarily brought the politics of race and class to the center of the nation's attention. Unfortunately, the political interest in the set of racial and urban conflicts that were highlighted by events in Los Angeles quickly faded. The Edsalls, Mickey Kaus, and Raymond Franklin each argues that the politics of racial and class conflict in America's urban areas should occupy the center of our attention both intellectually and pragmatically, because these phenomena fundamentally structure American politics, how Americans make day-to-day decisions on how they live, and the very nature of the republic in which we live. The political, economic, and social realities that were manifested in the spring 1992 violence in Los Angeles are viewed as symptomatic of a broad set of phenomenon and problems that are at the core of fears about American life for all-too-many residents of this country.

Black and white Americans view with dismay the desperate turmoil and decay that mark the nation's inner cities and fear increased racial conflict. Many are frustrated because neither the programs and policies associated with the Great Society nor the conservative regimes of Ronald Reagan and George Bush have seemed to slow the economic decline of urban America or the increasing fear of violent crime. However, it is clear that the political Right in this country has forged a formidable coalition between blue-collar workers and traditional Republican constituencies by being extremely successful at the state and national level in manipulating the politics of race and class. This coalition has successfully elected a Republican president in five out of the past seven elections, has transformed the Democratic solid South into a Republican bastion at both the state and national level, and has pried urban and suburban white members of the working class away from the Democrats. All three authors share a similar set of concerns, although they come to substantially different conclusions. They all want to encourage a revitalization of liberal and/or progressive politics. They are all concerned with the reemergence, both morally and practically, of a racially separate and unequal America that has developed over the past quarter-century through the mechanisms of white flight, residential segregation, privatization, and the "shrinking" of the public sphere. They all believe that the current state of the politics of race and of the underclass damages the republic. All three authors believe that there must be a fundamental change in values if current racial conflicts are not to polarize the country permanently. The authors disagree about whose values need changing (those of the Democrats, the black poor, or of whites). They disagree about who is responsible for what they see as the current state of racial turmoil and economic and social collapse in the inner cities. They disagree about what policy and political remedies are necessary to diffuse what they see as the crippling effects of the politics of race and the underclass, as well as about what set of policies can best address the needs the most marginalized African Americans and Latinos.

These books address a set of issues that, while largely ignored by political scientists, have been at the center of scholarly debate in all of the allied social sciences and the focus of policy debates at all levels of government and that have restructured the American party system. While the latter issue, in particular, has received some recent attention in political science, there has been significant neglect on the part of political scientists, particularly in the mainstream journals, on the political consequences of the intersection of race and the political economy. Unfortunately, despite their laudable intent, these three books share the same fundamental flaw. Each suffers from a myopia that allows each author, to different degrees, to present his assessment of racial politics authoritatively while ignoring critical voices. Two major categories of omission are found in these books. First, a significant body of research is ignored that undermines key assumptions made by the authors. Second, in works that are meant to explore the American malaise of the politics of race, we hear, by and large, only the voices of whites. The combination of these two flaws renders even the best of these three books fundamentally flawed.

Chain Reaction is the most serious of the three works. The Edsalls argue that the Democratic party has seriously undermined the quest for social justice and egalitarian principles by succumbing to the rights revolution and special interest groups and by being blind to the just concerns of white working people. By supporting racial preferences, higher taxes to pay for black programs, policies that are seen as violations of the norms of individual effort and hard work, and an ever-expanding welfare state and by being soft on crime, the Democratic party was unable to challenge a Republican onslaught at the national level. The Republican success was based on their ability to destroy the alliance among poor and working people by winning poor and working-class whites to the party by emphasizing the unfairness of Democratic policies that supported special interest groups such as women, labor unions, and (most importantly), African Americans. The Democrats fell into the Republican trap, according to the Edsalls, by being unwilling to examine critically the claims of special interest groups for rights, being unwilling to denounce black crime and social pathologies, and by being unable to understand that the burden of the policies they espoused and that were mandated by the Supreme Court fell hardest on poor, working-class, and lower-middle-class whites. Eloquent testimony by prosecutors in New York, precinct captains in Chicago, and the white grassroots throughout the nation is repeatedly used to demonstrate how far the Democratic party had strayed not only from key constituent elements but from American political values. The Democrats' problems were greatly exacerbated, according to the Edsalls, by a series of reforms (e.g., the McGovern Commission edicts) that removed profession politicians who were well connected to the white grassroots from the presidential selection process. The Edsalls first present their argument about what went wrong in the Democratic party and the implications for both politics and the nation and then provide several historical chapters that primarily trace how the Republicans are able to use racial conflict and polarization as a tool to build a national majority and greatly increase their power at the state level.

Most of this is synthesis of work done over the past several years by political and other social scientists. The book is particularly good at presenting the research that demonstrates the enormous gulf in black and white public opinion. The historical account is more accurate and detailed than that found in other recent attempts to describe the role of race in restructuring the American party system. However, the historical account, as well as the basic premises of the book, are systematically biased by presenting only the case of working-class whites. Voices of white residents of Canarsie are never contrasted with voices of black residents of Harlem. When combined with the selective use of research in this area, we are not allowed to understand the racial gulf in public opinion. For example, it is pointed out repeatedly and correctly that blacks support a much stronger and activist state than do whites. This is attributed to black dependence on welfare checks for the poor and jobs for the middle-class. Thus, the conflict is implicitly (occasionally explicitly) presented as one between blacks and their pursuance of policies that benefit their narrow self-interests and whites concerned with fairness and other American values. Similarly, white unhappiness with racial preferences is repeatedly emphasized as justified as an unfair burden placed on those who did not participate in various forms of racial discrimination that occurred in the past. What is left out is not only the reply of grassroots African Americans (black academics are usually used as the foil to counter the voice of "the people") but also substantial evidence from sociologists and economists that racial discrimination is pervasive in employment and housing markets. This evidence is so substantial that one of the main proponents of the view that contemporary discrimination plays a relatively small role in labor markets, sociologist William Wilson has come to the conclusion that discrimination in labor markets against blacks is increasing and is damaging black life chances. Evidence on the operation of loan, housing, and labor markets convincingly demonstrates that blacks and Latinos who try to get a job, find a place to live, or get a loan, will face negative outcomes because of their race even when matched with whites who share extremely similar profiles. When viewed in the light of this evidence, the difference between black and white attitudes toward the state and various racial policies is the result of a confrontation not between self-interest and principle but between different conceptions of racial reality, fairness, and different conceptions of the good society. As serious as is their effort and as laudable their goal, *Chain Reaction* presents a seriously distorted view of the politics of race within the United States.

Mickey Kaus's goal is also to understand the failure of liberal politics in the late twentieth century. Unlike the Edsalls, Kaus rejects the goal of trying to bring about a more equitable distribution of income. He argues, instead, for the concept of "civic equality." *The End of Equality* argues that income inequality is probably ordained to continue to grow and that redressing such inequality is not a worthy goal in and of itself. To bring back mutual respect between citizens and reconstitute the public sphere and public spaces should be the goals of liberals. Liberals must break the bond between money and how citizen's moral worth and dignity is judged. In his words, political equality is a necessary but not sufficient condition for the (re-)establishment of civic equality. Kaus's policy proposals, aimed at establishing equality between the classes in public life, fall into two broad categories. First, he argues for the strengthening of class-mixing institutions, such as the draft, national service, public schools, a semimandatory national health care system, and the reinvigoration of public spaces. These programs will not be sufficient, however, unless the problem of the underclass is confronted. Kaus argues that the underclass not only makes public spaces dangerous, driving the affluent to class segregated suburbs, but also taints all blacks with the brush of contempt.

Work is the means by which worthiness should be judged and by which Kaus believes that the underclass can be ended as a corrupting force that prevents an egalitarian society. He argues for mandatory employment of all able-bodied adults who cannot find work on their own, the establishment of universal day-care facilities, and a national health plan. He would eliminate all cash payments to those able to work. The jobs would pay a subminimal wage that would be supplemented by some system (he is vague) similar to an earned-income tax credit. These, as well as the national service youth, would participate in doing useful work, such as cleaning up public places, assisting the elderly, maintaining parks and libraries, and more generally reclaiming public spaces for citizens. He estimates the cost of this program, *before* the cost of a national health care system is counted, at approximately 150 billion dollars.

He forecasts that the end of the underclass will bring about not only the reconstitution of public life and the mixing of classes on a basis of equal dignity in the public sphere but would also have other benefits, such as the elimination of racism. However, American racism predates the establishment of the underclass by centuries. Kaus is much less careful than the Edsalls in building his case on solid research on race and society. Eliminating the economic and cultural marginalization of the inner-city poor is a critical task for this nation for reasons ranging from the stability of society to the hope of maximizing the possibilities of a fruitful life for as many residents of this nation as possible. The state of many of our inner cities represents both a national shame and a danger to the nation. However, it seems extremely unlikely that "solving" the problems of the underclass will eliminate racism—let alone promote residential mixing of classes and the spread of mutual respect among citizens—when survey data from the 1990 General Social Survey indicates that 54% of whites believe blacks to be less intelligent; when the work of Massey and Denton shows that blacks are the only group in America for which, as social status goes up, level of segregation does not go down; and when evidence on the persistence of racial discrimination in markets is considered. Kaus is also unfamiliar with the literature on how support for policies varies depending on how issues are framed. Further, he devotes remarkably little attention to the question of how such a set of policies that both are expensive and mandate a much higher level of state intrusion into every American's life would win political support. Kaus's book has some useful ideas about how what should be included in a much-needed national debate on equality and an equally needed debate on how to overcome the economic and social devastations that are all-too-common in American urban areas. Unfortunately, insufficient attention is paid to the critical connection between race, economics, status, and citizenship, to support his claim to have *the* solution both to the problems of liberal intellectuals and activists and to the problem of the underclass.

The main strength of Franklin's *Shadows of Race and Class* is that it seriously probes the interaction between race and class in American society. Franklin's main argument is that by only understanding how the overconcentration of African Americans in the "lower class" casts a stigmatizing shadow over all African Americans can one understand the dilemma of race in America. This shadow taints all African Americans with the characteristics ascribed to lower-class blacks. The qualifications of even the best-trained and best-placed blacks are considered suspect. Police harassment and (white) societal shunning are experienced by all classes of blacks, albeit to different degrees. Politically, he tells a similar story to the Edsalls' about how white backlash was a result of programs such as affirmative action and the promotion of ethnic diversity that were seen by most whites as inherently unfair and a violation of American political values. He traces the relationship between what he views as black subordination from slavery, through the debates on scientific racism of the nineteenth and twentieth centuries, to contemporary debates about the underclass. He ends by calling for a revitalization of American cities; the building of broad, multiracial, antiracist coalitions; a jobs program that would guarantee a job to every American; and a return to egalitarian antiracist studies.

Most of the themes he wants to address, particularly his insistence on the strong historical connection in the United States between matters of race and class, are exactly the ones that should be a fundamental part of the research and political agenda on race and political economy. However, the execution of the research is not adequate to the theoretical and political tasks that the author sets for himself. Much of the review of secondary material is both limited and badly out of date. In the chapter on slavery, the work of scholars such as Barbara Fields, Tom Holt, Jim Oakes, and Eric Foner is not cited. Data on "contemporary" public opinion on race is also badly out of date and very spotty. Worse, he makes a number of unsupported assertions that will not win him any converts. One typical example can be found in his discussion of race and biology. His discussion of different approaches to how one thinks about race, environment, and heredity is very useful. Unfortunately, many readers might have stopped reading as when they objected to his statement that all of those who adopt hereditary approaches do so to "as a screen to conceal racial biases. Since racially biased scientists are active respondents contributing to a racially biased social and class structure changing the former must go hand in hand with changing the latter" (p. 68). Even if one is sympathetic to such a claim—and many are—one would appreciate the presentation of at least some evidence on the attributions of the motives (as opposed to the results derived from research or the quality of the research).

Franklin is correct to emphasize the historical importance of white supremacy as an analytical construct for understanding the development of American racial conflict. He is also to be commended on arguing that one should study the changes in the national and international political economy to understand the racial dynamics of any particular period. Like the Edsalls, he attributes much of the current conflict to the squeeze being suffered by the white middle and working classes as the American economy is being reshaped in part due to increasing international economic competition. Unfortunately, his book will not win many converts to this approach.

Much is missed in these books due to their concentration on white perceptions of the politics and economics of race. One glaring omission is that there is no substantive discussion of the African American perception—typified by the King epigraph—that a significant part of the conflict is due to white reluctance to yield the fruits of centuries of supremacy. I am not arguing that this view should be more privileged than white views concerning black unfairness. However, to leave this view out is to render any discussion of these issues inherently inadequate for analyzing or solving America's racial conflicts. Many scholars have argued that the relatively recent phenomena of the underclass is so entrenched that they believe that even if the creation of such a class were due to changes in the structure of the economy, undesirable behaviors would persist even after the structural conditions no longer existed. White supremacy was a system of domination that lasted centuries, not decades. There have been *two* ten-year periods in American history—Reconstruction and Civil Rights eras—that saw rapid black progress in the face of stiff resistance. Why should one *not* expect (as most African Americans do) that both institutional and personal racism still plays a major factor in structuring the opportunities and lives of black people? At the same time that studies are confirming the ubiquitous presence of racial discrimination in all critical spheres of American life, other scholars are arguing for the repeal of *anti*discrimination legislation, on the grounds that discrimination is economically efficient and antidiscrimination principles constitute a violation one's basic property rights. Until discussions of race are both based on the best research and include the voices of not only blacks and whites but also the other voices that are part of the increasingly complex contours of the terrain of racial conflict, books such as these are more symptomatic of the problems of race and class than part of the solution.

University of Chicago MICHAEL C. DAWSON

Researching the Presidency: Vital Questions, New Approaches. Edited by George C. Edwards III, John H. Kessel, and Bert A. Rockman. Pittsburgh: University of Pittsburgh Press, 496p. $49.95 cloth, $22.95 paper.

Anthologies tend to be of two types. The first type reflects consensus within a field. Scholars have little difficulty agreeing that the topics discussed in the volume are the field's central concerns. The perspectives offered are also understood to be the dominant approaches of the day. The second type marks dissension within a field. The topics and perspectives of the volume show an area of study at odds with itself. The chapters reveal the central disputes, rather than the central agreements, within the body of research. Edwards, Kessel, and Rockman's edited volume is of this latter type. The book admirably shows the study of the American presidency in a state of flux and attempts more systematically than any other volume on the presidency to turn the field toward new theoretical directions.

The book grew out of a conference at the University of Pittsburgh in November 1990 that was designed to assess the current presidency literature and make recommendations for the future. The book is divided into three parts: the first takes stock of the extant presidency literature in the areas of election, personality and leadership, advising, and policymaking; the second offers new theoretical, empirical, and methodological approaches to the study of the presidency (e.g., cognitive theory, organizational theory, formal theory, the new institutionalism, applications of congressional leadership studies, and methodological issues of systematic inquiry); the final section presents a comparative perspective on the presidency examining presidents cross-nationally and evaluating their performance.

In the first section, the central contribution of John Aldrich's essay is twofold: its fine treatment of presidential election research is matched by its effort to show the relevance of the study of presidential elections to the study of the presidency. Aldrich suggests that in order to remove the intellectual barriers between the two fields, researchers should examine presidents' accountability for peace, prosperity, and tranquility. He views these three factors as the main expectations upon which the public judges presidents, the primary goals sought by presidential candidates, and the central aspects of the historical context that define any presidency or election. Although Aldrich acknowledges that the three factors are imprecise, their very imprecisions are a part of the political reality of the presidency and presidential elections.

Erwin Hargrove grapples with what is often perceived to be the biggest stumbling block to systematic research in the presidency—the significance of individuality. Hargrove attempts to carve out a middle ground between those who say personality is determinative and those who say it ought not be considered as relevant. Hargrove's most important contribution is in developing the notion of "political skill." He considers the repertoire of skills on which presidents are judged, including public presentations, conflict management in the administration and Congress, the organizing of groups, and negotiation with the executives of other nations. These aspects of behavior could be systematically studied across presidents and could resolve, in many ways, the extent to which individuality is important. Karen Hult too, indirectly deals with the concept of skill in her chapter on presidential advice. How presidents accept and reject advice and the advisory systems they devise are matters of skill.

Paul Light attempts to clarify policy as a dependent variable in presidency research. He contends that especially since Carter, short-term policy is more prevalent than long-term policy. Light's contention provides an interesting launching point for future research.

The second section offers a host of innovative avenues for presidency scholars to pursue. Barbara Sinclair brings her excellent work on congressional leadership to bear on the notion of presidential leadership. Of course, the translation cannot be exact, because congressional leaders hold formally designated party positions. But Sinclair concentrates on elements of purposive (i.e., rational) behavior of presidents and the skills they have to achieve their goals. Susan Fiske outlines social cognition theory and argues that presidents may behave as "motivated tacticians." In some cases, they make short-cut decisions by applying preexisting, generic categories of information (or schemas) to understand a new situation. In other cases, they are motivated to delve deeply into the situation to decide what to do. Martha Feldman and Terry Moe offer essays on organizational theory and institutional theory, respectively. Both present a view—which is becoming more commonplace in the field—that the presidency is an organization or institution with a life of its own, independent of the people who work within it, including the president. Gary King provocatively argues that much of the qualitative research on the presidency draws conclusions about the importance of individual presidents without judging the uncertainty of its inferences. He also asserts that models of presidential behavior should strive for parsimony, rather than inclusiveness.

Perhaps the most enlightening chapter in this section is Gary Miller's work on formal theory. Miller's application of formal theory to the presidency is less compelling than his well-crafted use of E. E. Schattschneider's concept of the socialization of conflict in considering the connections among public, legislative, and interest group politics in the presidency. From Schattschneider, Miller draws a distinction between normal and abnormal politics. In normal politics, conflict is largely private, interest groups settle deals in the halls of Congress, and presidents have little impact. In abnormal politics, presidents can use their public communication skills to socialize the conflict.

The final section involves two selections on comparison. Anthony King provides an array of features of executives that can be compared cross-nationally. Richard Rose discusses how success in office can be compared across presidents.

The most absorbing element of the book is found in the contrast between its first and second sections. The contrast brings out two of the central debates within the presidency field: personal versus institutional analyses and qualitative versus quantitative research. Hargrove, Sinclair, and Fiske express the importance of analyzing individual presidents from the proper theoretical perspective. Their essays advance, at least implicitly, the value of qualitative research in understanding the role of individual presidents. Hult, Feldman, Moe, and King do not find the behavior of individual presidents to be as important as the institution within which they operate. King, in particular, suggests that qualitative research too often lacks sufficient rigor. Thus, the book vividly portrays the dialectics of the field.

As exciting as this discourse is, the quarrels are some times exaggerated. The book reveals a kind of self-flagellation common among presidency scholars. The field, so the argument goes, is in its infancy. It has not yet broken out of its anecdotal, participant-observer approach to the subject. It remains fascinated by the differences across individual presidents far more than institutional similarities. Too often, it does not approach questions through quantitative analysis. Scholars are saddled with an inferiority complex that says that the presidency field is not as good as the Congress field.

Although these statements may have been appropriate in the 1970s and early 1980s, they no longer seem as telling as they once were. True, anecdotes still exist in the presidency field, but they also exist in the Congress field. Participant-observer methods are used perhaps more in the study of Congress than in the presidency. There have also been many illuminating studies of single members of Congress. It is no longer true that qualitative presidency research is unsystematic. With growing use of presidential libraries and other sources, scholars are carefully piecing together the private decision-making records of many presidents and reaching well-documented conclusions about them. Other research skillfully blends qualitative data sets with quantitative analytic techniques. Quantitative research, in general, is also more advanced than many passages in the volume imply. Unfortunately, the book's literature review section does not include surveys of the three key subfields of presidential popularity, presidential communication, and presidential relations with Congress. These areas are arguably the most highly developed in the presidency field, involving parsimonious models, long time-frames, quantitative approaches, and generalizations that transcend individual presidents. Had they been included in the volume, some of the disputes about the state of the field would have been less glaring.

Still, the book's display of the problems of the field and the controversies within it is healthy. It will stir new scholars entering the field to make certain that their research bears none of these difficulties. It also will push scholars already in the field to guard against a return to the old ways. This is a book that all presidency scholars should take seriously.

University of Arizona LYN RAGSDALE

The Supreme Court and Legal Change: Abortion and the Death Penalty. By Lee Epstein and Joseph F. Kobylka. Chapel Hill: University of North Carolina Press, 1992. 417p. $45.00 cloth, $16.95 paper.

The question this book addresses—What explains changes in legal doctrine?—has attracted legal scholars in every generation. According to Epstein and Kobylka, the traditional answer—the logic of the law—has been largely replaced by behavioral social science answers that highlight the political nature of judicial decision making and, accordingly, focus on changes in judicial personnel and judicial attitudes. Other recent work highlights the role of the broader political environment as the main causal agent of doctrinal change. However, Epstein and Kobylka reject these modern explanations (at least as sole determinants) of legal change. They return to the traditional answer and argue that social scientists have overlooked legal argument as an important, if not the most important, explanation of doctrinal change. As they put it, it is "*the law and legal arguments as framed by legal actors* that most clearly influence the content and direction of legal change" (p. 7, emphasis original).

Relying on a comparative case study approach, Epstein and Kobylka examine the modern history of litigation aimed at invalidating the death penalty and creating and maintaining a constitutional right to abortion services. While examining a number of cases, they isolate four: *Furman* (1972) and *Gregg* (1976) for the death penalty, and *Roe* (1973) and *Webster* (1989) for abortion. In each set of cases, they argue that the "initial case established a clear doctrinal change or innovation," while the later case represented a "sudden shift in legal result" (p. 6). Focusing on "legal, doctrinal change," they wish to explain both the innovation and the "turnabouts" (pp. 7, 9).

In examining death penalty litigation, Epstein and Kobylka argue that the legal argument made by the abolitionists in *Furman* "profoundly influenced the two pivotal justices—White and Stewart" to join three of their brethren in striking down existing death penalty legislation (p. 81). Similarly, abolitionists lost their battle to invalidate the death penalty permanently in *Gregg* because they failed to persuade White and Stewart. Abolitionists' "emphasis on the absolute and immediate eradication of the death penalty," rather than on the more cautious "practical, process-based arguments" that appealed to the two justices (pp. 134–35), spelled defeat.

In the abortion arena, while Texas prepared poorly in *Roe*, prochoice forces, through a coordinated litigation strategy, "shaped" the legal environment and "provided" the justices with "much of their ammunition" (p. 201). Prochoice forces lost in *Webster*, because they failed to capture two winnable votes, those of O'Connor and Kennedy. They failed because they neither responded in a serious way to Solicitor General Lee's unduly burdensome standard (first enunciated in *Akron*) nor reached out to try to win the vote of Justice O'Connor, perhaps by appealing to her "liberal" soft spot, gender discrimination (p. 296).

Epstein and Kobylka examine and reject other plausible explanations for the case outcomes, including changes in judicial personnel and the broader political environment (e.g., public opinion and the actions of political leaders and government officials). Rather, they conclude that both abolitionists and prochoicers lost cases principally because of their inability to shift the grounds of their legal arguments. Falling prey to the "tyranny of absolutes," "initial 'liberal' victories were forged and then lost, in significant part, because their defenders doggedly clung to their understanding of the Court's logic. This fatally constrained their ability to shift argumentational grounds when those victories came under threat" (p. 311).

The great strength of this book lies in the rich, detailed, and complex case studies that comprise over fourth-fifths of it. Epstein and Kobylka are at their best in describing the machinations, arguments, and decisions that formed the litigation strategy. They present the most thorough and accessible overview of the history of death penalty and abortion litigation that I know. Chapters are studded with excellent tables and figures summarizing issues, parties, arguments, case holdings, and so forth. The tables and figures alone make the book a worthwhile addition to one's bookshelf. It is also fun to read: Epstein and Kobylka manage to add drama to the story, even though the reader knows the outcome. In the tradition of Vose's *Caucasians Only* (1959), Epstein and Kobylka plum the complex interrelations of activist, interest-group, judicial, and governmental action in a fascinating and thorough way.

While the book's great strength lies in the richness of its case studies, its weakness lies in the application of the analytical framework to those studies. The authors address the book to the question of *why* the Court reached certain decisions, but it is more properly understood as an intensive study of *how* issues were presented to the Court. The theoretical argument as to why the Court reached certain decisions is less well developed and, accordingly, less powerful. In particular, Epstein and Kobylka do not fully explore alternate explanations, leaving their own position plausible but ultimately unpersuasive. Further, they claim causal influence without developing evidence to support more than correlation.

Because their main claim is that legal argument plays a major role in influencing case outcomes, they need to show how that influence works. Further, they need to develop a test and gather sufficient evidence to distinguish between their "legal argument" thesis and the competing explanations of Court personnel, political environment, and organized interests that they reject. For example, they credit the abolitionists' legal arguments with convincing Justices White and Stewart in *Furman*—and failing to convince them in *Gregg*—without providing any evidence other than their votes. They make a similar move in claiming causal influence for the legal arguments of the prochoice movement in *Roe*, and they suggest that a different argument could have carried O'Connor and Kennedy in *Webster*. Since the heart of their claim is that the legal arguments of the litigants are vitally important to case outcomes, these omissions render their theory ultimately unpersuasive.

Epstein and Kobylka devote insufficient space to exploring alternative explanations, particularly those that focus on the broader political environment. With the death penalty, an alternative explanation might point to factors like majority opposition to the death penalty in July 1966, rejection of it by Oregon voters in a 1964 referendum, an official request by the Johnson administration to Congress to abolish it in 1968, and falling execution rates throughout the 1960s, so that in 1968, for the first time in American history, there were no executions. After *Furman*, the story could not have been more different, the majority of states reenacting death penalties with the support of the U.S. solicitor general. A similar argument can be made with abortion. By the time of *Roe*, there was growing public support for abortion choice, little organized national opposition, widespread professional and elite support, and political reform leading to the performance of nearly six hundred thousand *legal* abortions in 1972, the year before *Roe*. After *Roe*, there was somewhat of a backlash, with the growth of an organized, national antiabortion movement and the enactment of restrictive legislation by a majority of states. With both the death penalty and abortion, the Court's decisions, it could be argued, simply mirrored these changes. While Epstein and Kobylka acknowledge these points, they provide no way of distinguishing their effects from those of the legal arguments.

There is also a question of whether *Gregg* and *Webster* represent as sudden and dramatic a departure from *Furman* and *Roe* as the authors suggest. *Furman* did not hold the death penalty unconstitutional per se; rather, it held the death penalty *as currently administered* unconstitutional. This strongly suggested that rewritten laws could be upheld. *Gregg* did just that. With abortion, *Roe* did not settle a whole host of issues from funding, to state provision of services, to spousal and parental consent. *Webster*, following a line of cases that Epstein and Kobylka canvass, filled in some of the gaps. In practice, it has made essentially no difference in the accessibility of abortion services. Thus, neither set of cases creates as dramatic a legal turnabout as Epstein and Kobylka argue. More importantly, both are consistent with some version of the traditional legal model, suggesting that they do not provide the best test cases for distinguishing between theories of legal change.

Overall, the lack of full theoretical development does not mar the depth and the richness of the empirical studies. *The Supreme Court and Legal Change* should become a standard text in courses examining litigation strategy. It also serves as a reminder to social scientists that the American judicial system, while clearly political, remains a *judicial* system immersed in the law.

University of Chicago GERALD N. ROSENBERG

Minority Representation and the Quest for Voting Equality. By Bernard Grofman, Lisa Handley, and Richard Niemi. New York: Cambridge University Press, 1992. 174p. $34.95.

Grofman, Handley, and Niemi have written a very useful descriptive and historical analysis of the litigation surrounding the 1965 Voting Rights Act and its subsequent amendments. The authors devote much of their discussion to explaining the implications of the 1982 Amendment and its relationship to the 1986 case of *Thornburg* v. *Gingles*. The *Gingles* case helped establish a set of standards for evaluating the dilution of minority votes. By 1986 litigants no longer had to prove discriminatory intent as a prerequisite to a challenge.

After acknowledging in chapter one that the right to vote in the U.S. has been largely achieved, the authors signal their intent to focus on second-order discrimination and unequal outcomes in which the candidates preferred by racial minorities lose at a greater rate than the candidates preferred by racial majorities. The central theme that runs throughout the book's remaining five chapters is that the Voting Rights Act was intended to do more than simply give minorities the right to cast votes. The Act was also intended to assure them certain outcomes. Here the authors disagree with the legislative history presented by Abigail Thernstrom in *Whose Votes Count? Affirmative Action and Minority Voting Rights* (Cambridge: Harvard University Press, 1987), and with others who argue that the 1965 Voting Rights Act was passed solely to guarantee black enfranchisement.

Grofman et al. argue that legislators who debated the merits of voting rights legislation were keenly aware that white racists would attempt to circumvent the law, by means such as at-large voting, runoff primaries, and gerrymandered districts. As a consequence, they designed the law with minority representation in mind. For these authors, minority representation is purely descriptive in nature and it occurs when elected officials belong to the same racial group as their constituents.

As is typical of advocacy literature, the authors go to great lengths to avoid admitting the significant progress that minorities have made in the political arena. After dismissing as aberrational the increasing number of black politicians elected in mainly white jurisdictions at all levels of government, we are told that at the congressional and state legislative levels, blacks are very unlikely to be elected from districts that are not majority minority (p. 134). Never mind that in 1990 forty percent of the blacks in Congress were elected from districts that were not majority black in their voting-age populations. After conceding the limitations of drawing additional majority-minority districts, the authors tell us that we must nevertheless continue along the same path because present day racial polarization precludes the establishment and maintenance of multiracial coalitions.

Ignoring evidence to the contrary, and thoroughly rejecting any notion that white politicians might be able and willing to represent blacks, we are told that white liberals are too risky because they may not have the same commitments to civil rights and economic issues as a black leader and that white representation of blacks is a "variant of a very old style of paternalism." White incumbents who wish to retain their elected offices are "factor(s) retarding black representation." (p. 135).

In advancing this narrow view of representation, the authors come dangerously close to stating that blacks must be represented by blacks, Latinos by Latinos, and Asians by Asians. They fail to note the distinction between the real paternalism which existed before the passage of the 1965 Voting Rights Act, when blacks were dependent on the goodwill of white politicians, and today's more symbiotic relationship in which white incumbents, especially Democrats, need black support.

Minority Representation and the Quest For Voting Equality was published before the Supreme Court decision in *Shaw v. Reno* (June 1993), which may prove to be the most important voting rights case of the decade because it expands the discourse surrounding race and representation and spawns a host of new questions. In *Shaw* the Court used a challenge by white voters to North Carolina's 12th district to call into question all race-conscious districting plans. Reacting to the creation of a serpentine district that winds 160 miles along Interstate 85, Justice Sandra Day O'Connor, writing for the majority, proclaimed that "racial gerrymandering, even for remedial purposes, may balkanize us into competing fractions; [and] it threatens to carry us further from the goal of a political system in which race no longer matters."

Despite disclaimers, the authors believe that proper enforcement of the Voting Rights Act would require the proportional representation of minorities. But as astute social scientists, they recognize the limitations of proportional representation and the unlikelihood of adoption in the United States. As a consequence, we are told that although "Blacks and Hispanics cannot expect to have a number of representatives equal to their percentages in the population," [this] "does not mean that any disproportionate results are acceptable or even that moderately disproportional results are acceptable" [because] "neither minorities nor whites/Anglos should be content with minority percentages much below their proportions of the population." (p. 133).

The primary strength of the book is its detailed and useful account of the statistical procedures accepted and rejected by the courts during the 1980s and its discussion of vote dilution. However, it does not define representation and its concluding chapter is too short and too cursory to enhance our understanding of minority representation and its complexity. Nevertheless, this important book should be read by anyone interested in learning more about the role of social scientists as expert witnesses and about the pre-1990s voting rights cases.

Princeton University CAROL M. SWAIN

Politics in the Lifeboat: Immigrants and the American Democratic Order. By John C. Harles. Boulder: Westview, 1993. 245p. $49.95.

This ambitious study expands our understanding of ethnic political incorporation in the United States. Nonetheless, the author arrives at exaggerated or indefensible conclusions about the contribution immigrants have made to American democracy. Its conclusions notwithstanding, Americanist and those studying any polity experiencing substantial infusions of refugees and immigrants should find this volume useful.

The study reexamines the claim that ethnic heterogeneity causes instability—a proposition with extensive empirical support and one that analysts since Aristotle have made into a truism. In ethnically diverse societies, distinct populations are unlikely to share fundamental values and may literally be unable to communicate with each other. Societal arrangements in multiethnic societies also tend to reward one group at the expense of others, thus exacerbating the extent to which groups see themselves as distinct. Politics in ethnically heterogeneous polities, thus, often reflects reenforced cleavages that produce intense conflicting claims, many of which require zero-sum solutions. By contrast, cultural homogeneity facilitates communication and produces consensus regarding core social and political values, which, in turn, enhances the likelihood that the political problems are amenable to compromise. In sum, heterogeneous societies face problems including the threat of dismemberment that homogeneous societies never experience.

How, then, given the ethnic diversity of the United States, can American political stability and democracy be explained? The answer lies in the nature of American ethnicities. Analysts have long assumed that all ethnically diverse societies consist of culturally distinct populations with a history of distinct allegiances and competing territorial claims that are likely to be a continuous source of instability.

However, the author correctly points out a simple truth that most analysts have ignored: all ethnically diverse societies are not similarly constructed. In the United States, immigration is the principal source of ethnic diversity. The relationship among American ethnic groups thus differs profoundly from what exists in societies where ethnic diversity reflects historical conflicts. The author argues that it is incorrect to assume that ethnic heterogeneity originated by immigration will have the same political consequences as diversity which results from conquest and subordination. The differences in these ethnicities explains why it is inappropriate to speculate about ethnic politics in the United States based on the experiences of countries such as the former Yugoslavia, Belgium, and India.

Acknowledging that in the latter types of societies, diversity is a major source of instability, the study claims that immigration-generated heterogeneity has enhanced American political stability and democratic institutions. To support this thesis, the author makes a two-part argument. First, he argues that immigrants are not a threat to the polity. Instead, they are predisposed to being strongly patriotic, whether or not they support the liberal democratic values that make up the American creed. Moreover, immigrants are so disinclined to be politically active that even if they do not support these core values, they will not act so as to threaten the system. Second, their quiescence and unequivocal patriotism make them the source of much of the diffuse support needed to maintain the nation's democratic order.

The first part of this argument is insightful and persuasive. Using a variety of historical sources to illustrate his theoretical argument, the author points out the irony of opposing immigration on the basis of questioning whether immigrants will transfer their loyalties to the United States.

The second part of the thesis falls under scrutiny. To show that quiescent immigrants have enhanced stability and democracy, the author needs to show that the nation's political institutions would have evolved in undemocratic directions in the absence of immigrants. He offers no such evidence, however; nor does he ask the question. Furthermore, while immigrants have not directly threatened the nation's political institutions, depending on their numbers, residential patterns, and how society perceives them, they have exacerbated or given rise to conditions that jeopardized democratic procedures and stability. For example, in the Southwest, the political passivity of the majority of Mexican immigrants combined with anti-Mexican attitudes in society to entrench a fundamentally undemocratic political order. Efforts to correct those processes have produced intense local-level violence that had national ramifications. Similarly, recent riots in Washington and Los Angeles illustrate the destabilizing potential of large numbers of residents who, despite their "patriotism," live in extreme poverty and have no mechanisms for making government attentive to their problems. Again, Cuban quiescence regarding undemocratic racial politics contributed to maintaining such practices in Miami well after they were abandoned by other southern cities—practices that contributed to political instability as evidenced by race riots in recent decades. Examples such as these indicate that contrary to the author's claims, it is essential for immigrants to be socialized into American political norms for the polity to maintain its democratic commitments. The author both undervalues the significance of such socialization and the pace at which it occurs.

The study suffers from two additional major problems. First, the original data it analyzes is from interviews with thirty Laotian refugees. The author acknowledges, but understates, the limited utility of his sample. More problematic, however, is the claim that conceptually, Laotian *refugees* are representative of *immigrants* per se—that even though they are refugees, their views toward the polity will not differ from those of immigrants. The author's arguments defending this assertion are unpersuasive and seem almost contrived.

University of Texas, Austin RODOLFO O. DE LA GARZA

Democratic Values and Technological Choices. By Stuart Hill. Stanford: Stanford University Press, 1992. 267p. $37.50.

Stuart Hill has successfully used concepts from cognitive social psychology, specifically schema theory, to construct a more accurate explanation of citizen assessment of technological choices—referred to as "social process" theory. He has produced a superior piece of public policy analysis by filling in the lacunae in previous research on citizen assessment without rejecting such analyses entirely. He uses the past-structured assumption of prescriptive analysis as a baseline. He never dismisses it entirely; in fact, his chapter on political veterans actually does much to strengthen the argument of prescriptive policy analysts by underscoring that such individuals are shaped by general, substantively oriented perspectives from the past. Political activists have assessments of a specific case that are far more extreme that those of the general public. Secondly, because their assessments are shaped by past experiences, case-specific issues and procedural judgments have little impact on their substantive conclusions about technological choices. In this regard, most prescriptive analysts could only nod their heads in agreement.

However, Hill's main contribution to the study of technological assessment arises because he has the intellectual curiosity to move beyond the realm of political veterans and investigate the potentiality of the general public's skills and capabilities. He develops a present-structured or "social process" theory which suggests that the general public, as contrasted with political veterans, can and will develop complementary skills, but do so at a latter stage in the process, during the course of public debate on a technological choice.

Hill does not dispute the widely-held view that the general public's level of information and background is limited; what he does reject is the view that the general public is incapable of reacting to public debate in an effective and responsible manner. This distinction becomes even more crucial when one centers on public policy involving major technological choices. In this policy arena, Hill successfully argues that there are strong incentives for the general public to pay attention to a well-defined debate, and, as a result, they often have clearly defined, informed responses.

Because Hill's "social process" theory distinguishes the behavior of political veterans and the general public, it represents a challenge to the assumptions of current policy-analytic techniques—cost-benefit analysis and decision techniques—which deny that experience plays a significant role in shaping evaluative procedure or that different types of individuals could use different methods of assessment over time. What Hill has so skillfully done is offer a more valid, and therefore more complex, picture of citizen assessment that takes into account the effect of both past and present deliberations.

The empirical sections of Hill's book represent a straightforward use of Q methodology in analyzing the data obtained by a survey of registered voters living within twenty-five miles of the Diablo Canyon Nuclear Power Plant in central California. Both the path analytic and in-depth interview sections give considerable support to Hill's conclusion that the lack of extensive background did not prevent most of these citizens from developing constructive arguments about an issue that was highly salient to them.

In his final chapter, entitled, "Taking Stock," Hill underscores that importance of the policy analyst's choice of a method and an evaluative framework. Each choice indicates the implicit or explicit expectations about the strengths and weaknesses of citizen assessment, and, as a result, such premises constrain the means that will be considered, and so indirectly may affect the end. Hill's contribution is that he has established a strong theoretical and empirical basis for higher expectations of the general public's capabilities for technological assessment, and therefore has presented a cogent argument for removing the constraints that have often limited public involvement in technological choices.

University of Oregon PRISCILLA L. SOUTHWELL

The SEC and Capital Market Regulation: The Politics of Expertise. By Anne M. Khademian. Pittsburgh: University of Pittsburgh Press, 1992. 278p. $39.95.

The study of regulatory policy, like most fields in political science, is subject to intellectual trends. For decades, many scholars accepted the proposition that regulatory agencies were inept, rigid, and captured by the very interests they were charged to regulate. More recently, many policy analysts have embraced the political control thesis. Employing principal–agent models and the assumptions of rational choice theory, many now argue that the political control of regulatory agencies by Congress or the president is both effective and efficient. Khademian's *SEC and Capital Market Regulation* provides a detailed and lively examination of an agency that is driven by expertise and frustrates most simplistic notions of agency capture or political control. Instead of a moribund, captured regulatory commission, Khademian presents an agency that actively prosecutes violations of the securities laws with the support of an industry that consistently advocates vigorous enforcement. Of course, none of this should be a surprise. The Securities and Exchange Commission is unusual among regulatory agencies due to its long history of strong leadership, legal professionalism, and prosecutorial zeal. The real contribution of Khademian's book is to reveal that simple models of political control or congressional dominance simply do not suffice in describing the SEC's relationship to Congress. Khademian explains: "Congress defers to the expertise of the SEC because of the technical and uncertain nature of securities policy, the diversity of regulated interests, and Congress's dependence on the SEC to enforce the securities laws. The result is a balance between the respect for expertise, on the one hand, and the demand for accountability to elected officials, on the other, that gives the SEC and its political overseers mutual and reciprocal influence over the framework . . . for regulating securities" (p. 210). While Khademian devotes much of the book to an examination of the formal and informal institutions that have evolved to govern the relationship between Congress, the SEC, and industry actors, she carefully explores the symbiotic relationship between Congress and the SEC and how the regulatory framework integrates the interests of a variety of actors.

The SEC and Capital Market Regulation begins with an insightful introduction addressing the key features of the SEC and the securities industry. One of the central goals of the book is to illustrate the limitations of political control through an examination of the SEC and the institutions of securities regulation. This is carried out through the remaining chapters. Chapters 2–3 provide a brief history of the SEC and the legislation that created the prevailing model of regulating securities through the disclosure of information and progressively expanded the agency's jurisdiction. Chapter 4 examines the role of disclosure and enforcement in securities regulation, focusing on how the SEC's approach to regulation has been reinforced by the professional norms of the agency's legal staff while meeting the needs of Congress, self-regulating organizations, and a heterogeneous industry. Chapters 5 and 6 explore the history of the SEC and securities regulation during the 1970s and 1980s. Chapter 6 is particularly interesting in its analysis of the Reagan administration's attempts to introduce deregulation and regulatory reform by violating established agency norms, enhancing the role of economic analysis in agency decisionmaking, and altering budgetary and enforcement priorities. Khademian explains how these attempts were frustrated by the SEC staff and Congress and how the new budgetary climate of the 1980s threatened Congress's relationship with the SEC. Chapter 7 analyzes the conflicts between Congress and the SEC over the Reagan administration's attempts to reduce the costs of complying with disclosure requirements through the introduction of electronic information transmission and parallel efforts to increase the role of economic analysis in policymaking. The book concludes with a discussion of the SEC as a regulatory agency and the various factors that could contribute to change in the future.

Given the overwhelming success of Khademian's book and her effective use of interviews with past and present participants from the SEC, Congress, and the industry, one is hesitant to identify areas that are lacking. However, I note two weaknesses. First, the author explicitly frames the study in reference to prevailing models of political control, which, she believes, underestimate the power of the bureaucracy and the importance of expertise, when she might have devoted greater effort to the task of revising the theory of political control in light of her rich case study. Second, a more developed examination of expertise and professionalization would have been desirable. The author carefully documents her thesis that the SEC's legal expertise provides the basis for its regulatory success and political independence. However, she might have included a theoretically grounded discussion of professionals, professionalization, and the problems that exist when experts are placed in an organizational context. The vast literature on this topic is unexplored. Moreover, although the author presents the professional norms and career incentives of the legal staff, she does not devote sufficient attention to the economists who have become more important in recent years. She addresses, in general terms, how economists view markets, their support for deregulation and regulatory reform, and their growing role during the Reagan administration. However, this discussion could have been combined with a rigorous examination of dominant economic theories of capital markets and capital market regulation. Khademian's obvious competence in leading us through the complex world of securities regulation suggests that she is also capable of opening the complicated world of financial economics to her audience.

These final points should not detract from Khademian's accomplishment. She has written an animated, sophisticated, and accessible analysis of securities regulation—a field dominated by narrow specialists and securities lawyers. *The SEC and Capital Market Regulation* joins Shapiro's *Wayward Capitalists* (1984) and Seligman's *Transformation of Wall Street* (1982) as one of the definitive works on securities regulation appealing to a broad audience. It is also a welcome addition to a growing number of analyses of regulatory decisionmaking and administration. It should be required reading for anyone attracted to, or disturbed by, prevailing models of political control and bureaucratic decisionmaking.

Wesleyan University MARC EISNER

Ideologues and Presidents: From the New Deal to the Reagan Revolution. By Thomas S. Langston. Baltimore: Johns Hopkins University Press, 1992. 247p. $35.00.

Regardless of how one feels about its legacy, the presidency of Ronald Reagan has excited scholarly interest like few other administrations. One of the Reagan administration's most controversial features was its intense ideologization. Thomas Langston argues that far from being a unique feature of the Reagan era, the important role played by ideologues was the culmination of a significant trend that began with Franklin Roosevelt and continued through a transitional stage during the administration of Lyndon Baines Johnson. Ideologues have become both "increasingly indispensable and irresistible to presidents who expect their administrations to bring about significant change" (p. 18).

These three "presidents of achievement"—a category introduced by Erwin Hargrove and Michael Nelson in *President's, Politics and Public Policy* (1984)—exhibited the personal ambition and commanded the political resources needed to undertake a fundamental redirection of American domestic policy. In this view, brief bursts of dramatically reorienting legislative activity are followed by "presidencies of consolidation" while those that lay the preliminary groundwork in public and elite debate are "presidencies of preparation." Langston extends the Hargrove and Nelson analysis by providing new insight into how presidents may still pursue dramatic departures in domestic policy despite the decline of political parties.

Traditionally, policy realignments launched by presidents of achievement hinged on a partisan realignment. But absent such a shift in popular partisanship, a phenomenon much examined in the scholarly literature, recent presidents of achievement have been forced to find an alternative means to pursue their ambitious programmatic goals—reliance on "people of ideas" or ideologues. Thus, Langston suggests that presidents committed to pursuing a goal of massive policy change may pursue a partisan-based strategy or an ideology-based strategy, but only the latter has been available during the recent era. In the last three presidencies of achievement, "people of ideas . . . have become . . . more systematically integrated into White House operations and more closely tied to the defining domestic interests of these administrations" (p. xvi). Moreover, suggests Langston, the trend is likely to continue with serious implications for democratic accountability.

In a field largely defined by the work of Richard Neustadt and its well-known focus on presidential persuasion, ideology has received relatively little attention either as a means to motivate associates within the presidency and executive branch or as a basis for appealing to elites and the public. Langston hopes to bring "ideology into the mainstream of studies of the presidency" (p. 174). Toward this end, he develops and applies an "ideal type" to identify ideologues and then examines their impact on important domestic policy initiatives for each of the three administrations. Potential ideologues are evaluated in relation to four ideal characteristics: expressing claims of logical certitude, absolute truth and comprehensive explanation; ascribing value to groups experiencing different social conditions, that is, the tendency to dichotomize people; displaying an immanent historical consciousness as expressed in the blueprints for analyzing and resolving problematic social relations; and possessing a link to the institutional apparatus undergirding an ideological movement. This ideal type is applied to both individuals and groups who served within the three administrations. One of the book's most interesting discussions (chapter two) focuses on how these people were recruited and managed by each president, with the Reagan experience given particular attention.

The three case studies are based on secondary sources, analysis of documents found in the Roosevelt and Johnson archives, contemporary commentary, and more than fifty personal interviews conducted by the author. Roosevelt's establishment of the Tennessee Valley Authority, Johnson's Model Cities program and Reagan's 1981 initiatives to establish supply-side economics are the three cases. The central analytic focus is on the role played by people of ideas: the two competing ideologies and personalities whose struggle doomed TVA; two groups of administration liberals whose conflict complicated implementation of Model Cities; and a largely cohesive group of supply-side zealots whose unity diminished after 1981. More importantly, however, Roosevelt and Johnson remained closely attuned to the partisan forces that underlay their electoral success, while Reagan operated more like an apartisan president. Roosevelt gave little validity to ideological reasoning, Johnson encouraged the generation of ideas but subordinated them to his partisan goals, and Reagan steadfastly pursued an ideological goal with little grounding in popular support.

Langston's emphasis is on a distinctive subset of modern presidents. Presumably, if presidents of achievement turn to ideologues as a means to fill the vacuum in partisan politics, the same forces should compel presidents of preparation and consolidation to do so as well. Langston briefly surveys evidence on other administrations (Bush is given lengthier discussion in the Epilogue) but leaves the issue unresolved, a critical question if this is truly a long-term trend affecting the office. Although Langston offers an illuminating treatment of Reagan, it is reasonable to question whether the trend is likely to continue. Reagan's roots in partisan politics were distinctively shallow, so much so that it seems premature to predict that any president will soon enjoy comparable freedom to ignore partisan forces.

It is also important to note that modern presidents have sought to fill the partisan vacuum through means other than turning to ideologues. As Samuel Kernell has demonstrated, *Going Public* (1986) has become a favorite presidential strategy pursued by broadcasting appeals for broad support. Presidents dating back at least to Franklin Roosevelt have also used a variety of strategies to create temporary coalitions of like-minded interest groups, constructs termed "presidential parties" by some analysts. Reagan made extensive use of both these techniques to advance his agenda, as well as relying on the contributions of ideologues. Thus, a reliance on ideologues may merely be one of several means employed by modern presidents to pursue their programmatic purposes.

Langston's focus on ideology as an elite phenomenon departs from a major trend in the literature evident in works such as Theodore Lowi's *The Personal President* (1985) and Barbara Hinckley's *The Symbolic Presidency* (1990) which have stressed the overriding importance of public expectations in presidents' conduct of the modern office. Much recent scholarship has focused on the need of presidents to fill the gap between inflated expectations and reality. Filling that gap may lead presidents to rely heavily on ideologues to develop and articulate a credible case for programmatic achievement, but that role may be significantly different from the one played by ideologues in presidency's of achievement. Like Langston, both those authors focusing on alternative means to fill the vacuum of partisanship and those concerned with public expectations have concluded that policies developed without the benefit of partisan support are more likely to be unstable and subject to sudden shifts. These patterns trigger similar concerns about accountability as well. Thus, Langston adds to a growing body of literature dealing with these larger topics of concern for a democratic polity.

University of Delaware JOSEPH A. PIKA

Foreshadows of the Law: Supreme Court Dissents and Constitutional Development. By Donald E. Lively. Westport: Praeger, 1992. 168p. $49.95 cloth, $17.95 paper.

This book is aimed at students, not at scholars. Its seven chapters survey major controversies in American constitutional history—slavery, economic liberties, race, freedom of speech, and the like. The author does not claim to offer new research; rather, he seeks to provide students with an overview of important Supreme Court rulings and the context in which they occurred. He also includes generous excerpts from the judicial opinions he discusses.

What ties the various chapters together is the author's focus on the subsequent vindication of dissenting opinions. In adopting this approach, he has taken his cue from Chief Justice Charles Evans Hughes, who once described a dissent as "an appeal to the brooding spirit of the law, to the intelligence of a later day." Lively, however, never clarifies his view of the connection between the dissents and subsequent jurisprudential developments on the Supreme Court. If he is only asserting that the positions taken in some dissents were eventually adopted by Court majorities, the claim is incontrovertible but also trivial. In the absence of some causal connection between the dissent and change on the Court, the dissents become merely a rhetorical "hook" for a standard account of constitutional history. Yet if he is asserting that the dissents produced the changes on the Court, that the arguments of the dissenters persuaded the Court to alter its course, as the volume's title seems to imply, then that more interesting claim can be verified or disproved. What is striking is how little support the book provides for that claim. Let us look at the examples used by the author.

In some instances, a constitutional change consistent with the dissenters' views in fact occurred but did not, at least initially, take place on the Court. For example, following the Civil War, it was the Fourteenth Amendment, rather than the Court, that repudiated the view of United States citizenship adopted in *Dred Scott* v. *Sandford*. If anything, the Court attempted to circumscribe the effects of the Fourteenth Amendment in the *Slaughterhouse* cases. Moreover, there is no evidence that the dissents by Justices McLean and Curtis in *Dred Scott* significantly influenced the congressional debates leading to the proposal of the Fourteenth Amendment.

In other instances, there was a doctrinal shift on the Court, but factors other than the persuasive power of a dissenting opinion accounted for the shift. A pertinent example is the Supreme Court's treatment of substantive due process. When the Court embraced substantive due process in *Allgeyer* v. *Louisiana*, after initially rejecting it in *Slaughterhouse*, the change in the Court's position owed far more to the persuasiveness of Thomas Cooley's *Constitutional Limitations* and to the triumph of laissez faire than to the dissents of Justices Bradley and Field.

In other instances, the claim that the Court embraced the dissenter's views is itself controversial. For example, did the Court in *Brown* v. *Board of Education* really adopt the idea of a "color blind Constitution," put forth by Justice Harlan in his celebrated dissent in *Plessy* v. *Ferguson*? Although Lively assumes that it did, the Court in *Brown* never explicitly overrules *Plessy* and never employs Harlan's "color blind" language. The connection between Harlan's dissent and Chief Justice Warren's opinion in *Brown* is more complex than Lively implies.

Finally, one example offered by the author seems altogether inappropriate, since it does not even involve the vindication of a dissent. Rather, Lively merely shows how Chief Justice Taney repeats in *Dred Scott* the views he expressed in his concurring opinion in *Prigg* v. *Pennsylvania*. His inclusion of this example provides a basis for discussing slavery as a constitutional issue, but it hardly advances the discussion of the influence of dissenting opinions.

In fairness, Lively's volume does demonstrate that dissents sometimes influence the course of constitutional development. The Court's acceptance of the clear-and-present-danger test can be attributed to the forceful dissents of Justices Holmes and Brandeis during the 1920s. Similarly, its recognition in *Katz* v. *United States* that electronic eavesdropping implicates the Fourth Amendment owes much to Brandeis's dissent in *Olmstead* v. *United States*. Nevertheless, even in those instances which arguably support his thesis, Lively has the tendency to push his argument too far. *Olmstead* may lead to *Katz*; but does it necessarily lead to *Roe* v. *Wade*, as Lively suggests? Even if Harlan's dissent in *Plessy*, arguably, is vindicated in *Brown*, can one also claim, as Lively does, that it is likewise vindicated in Supreme Court rulings upholding affirmative action? Indeed, it is opponents of affirmative action, like Justice Scalia, who champion color blindness as a constitutional standard, while proponents of affirmative action tend to dismiss color blindness as an inadequate response to the nation's long history of racial oppression.

Foreshadows of the Law, in sum, does not prove that dissenting opinions have had a major influence on the development of American constitutional law. Indeed, I came away from the book less convinced of their influence than when I began the book.

Rutgers University, Camden G. ALAN TARR

Changing Patterns in State Legislative Careers. Edited by Gary F. Moncrief and Joel A. Thompson. Ann Arbor: University of Michigan Press, 1992. 237p. $37.50.

This collection of work by 15 political scientists is a fine example of contemporary research on state legislatures. As the title indicates, the volume explores how patterns have been changing, in terms of legislative careers, both electorally and institutionally. Allowing for the wide variation in state legislatures—from the full-time careerist bodies in California, New York, Michigan, and Pennsylvania to the part-time citizen bodies in South Dakota, Wyoming, New Hampshire, and Vermont—this research takes off from the major themes of professionalization, institutionalization, and modernization. It recognizes that institutional development has had a significant impact on incentive structures and career patterns, which, in turn, have left their mark on the legislature as an institution. Nor is it any accident that as state legislatures have exercised a greater ability to recruit members, they have become, organizationally, more like the U.S. Congress.

Just as legislatures seem to be following in the footsteps of Congress, so state legislative research is pursuing questions that have been dealt with by congressional research. Thus, studies in this volume focus on background characteristics of legislators, the electoral advantages of incumbency, campaign finance, and committee assignments (among other subjects). But those who examine state legislatures have one significant advantage over their congressional-research colleagues: they have many more cases at their disposal. The contributors to this volume make good use of the number of legislative bodies that can provide grist for their analytical mill, and the grist is of high quality.

The book, thus, testifies to the success of researchers in this field in collecting, developing, and analyzing empirical data. The variety of data exploited in these studies is impressive indeed. Thompson and Moncrief, in their piece on nativity and mobility, draw on documentary information on over four thousand legislators from 30 states. Patricia Freeman and William Lyons rely on a mail questionnaire to members of houses in four states to discover whether there is a new type of woman in public office. Harvey J. Tucker and Ronald E. Weber use election returns from 18 states to explore system-versus-constituency competition. David Breaux and Malcolm Jewell also use electoral data from 18 states to test the power of incumbency. Anthony Gierzynski and Jewell make use of data on legislative campaign finance from 7 states in their study of legislative caucus and leadership campaign committees. Ron Hedlund draws on information regarding the committee requests of legislators in four states to shed light on the assignment of members to standing committees. All of this should show political scientists that the legislative data sources are far from exhausted, with new ones constantly being discovered.

Not only have the contributors been industrious in collecting data, but they have also been skillful in analyzing it. They do not have to resort to indicators or surrogates, nor do they have to infer much or stretch very far. These studies are solid and direct, a model of their kind. They serve to test our beliefs about legislative life, confirming some of these beliefs, qualifying others, and offering a few that we may not have considered very much at all.

Confirmation of what we already believe to be so may seem to be much ado about little, but it is by no means a trivial matter. Rather, it is a worthwhile product of empirical research. Of significance in this regard is the finding from the Tucker and Weber study that while competition at the district level is declining, control of legislative chambers has been becoming more competitive. Districts may be safe for more incumbents, but fewer legislative chambers are safe for one party or the other. However, the percentage of marginal seats in many legislative house (e.g., Colorado, Connecticut, New Jersey, Oregon, West Virginia, and Wyoming) is over 50. This is much higher than we might have imagined, a single but not at all obvious discovery. Of major significance also is the finding by Gierzynski and Jewell that legislative party campaign committees target their resources to close races and are more likely to give to nonincumbents than to incumbents. It would appear sensible that legislative parties concentrate their resources where they are likely to be most effective; but without this research, we could not be at all sure that they do so. Peverill Squire's overview of leadership careers provides confirmation for some earlier work, demonstrating that the number of leadership positions in many states has been increasing and the tenure of house speakers has been on the rise. This, too, is a very useful contribution to our knowledge.

Several of these studies serve to qualify the beliefs we currently hold. Breaux and Jewell, for example, demonstrate that 8 or 9 incumbents out of 10 tend to win reelection, something that we already know. But they also show something that we definitely did not know, namely, that the incumbency advantage has not been increasing lately but, rather, that there has been no real change in reelection rates since the late 1960s. Although the power of incumbents has been increasing, the change has been manifested not in election victories but in the bigger margins incumbents receive in winning.

At least one of the findings—by Freeman and Lyons—runs counter to our intuitions and prior knowledge. It is that women experience less conflict between political life and family obligations than do men. This is a most interesting piece of information and requires greater explanation than the authors give. Interesting also is Moncrief and Thompson's discovery that increasingly, migrants to a state are getting elected to the legislature—and disproportionately to the minority party. They suggest that these nonnatives are likely to be agents of change, which would necessitate further research.

In traditional fashion, the editors conclude their splendid essay on the development of state legislatures with a call for further research. The work represented in this volume reminds us how much remains to be done. Our consciousness about the committee assignment process has been raised. That leads to questions as to variations between major and minor committees, the selection of chairpersons, and the differences that assignments make to the legislative system. Our understanding of leadership tenure is greater; and that brings up questions of the role and power of leaders, including variations from state to state, between chambers, and over time. Without doubt, we now have a much better idea of legislative careers, which prompts us to ask about the consequences of careerism for the structure, functions, and standing of the state legislature. It is easier to raise questions than to answer them. This volume has managed to do both.

Rutgers University ALAN ROSENTHAL

Passions and Interests: Political Party Concepts of American Democracy. By Gerald M. Pomper. Lawrence: University Press of Kansas, 1992. 178p. $27.50 cloth, $12.95 paper.

Architecture governs here: I cannot recall a scholarly book in political science with so many categories and so much interrelating of categories in so few pages. The result is a deductive rigor and tightly reasoned argument that leaves the reader no room for nodding or skimming.

Employing three dichotomous dimensions of political parties, Pomper generates eight master concepts (2 × 2 × 2). Some relate differently to the American party experience than do others. Pomper recognizes, for example, that the models of the party as either social movement or ideological community apply only to minor parties. Together, the eight form the basic structure of the book. Pomper also posits three dimensions of "democratic theory"; and they, too, generate eight different relationships between party and democracy, one each for the eight concepts of party, *mirabile dictu*. Later on, the analysis is augmented by four types of voter behavior and four categories of proposed reform of the parties, each of which is related to the eight concepts of party.

Architectonics aside, five of the book's nine chapters are devoted to analyses of the eight master concepts and their applicability to the American parties through their long history. They are the most persuasive part of the book, containing the reflections and the insights of one of the major scholars and most passionate defenders of the American parties. The eight function collectively as a prism through which one sees different characteristics of those parties at different times and under different circumstances. The constant attention to the relationship of party and democracy honors a tradition rooted in the great works of V. O. Key and E. E. Schattschneider.

While the author disclaims any attempt to build a theory of the American parties (p. 2), he does sidle up to one at several points. In so formally ambitious an essay, moreover, readers will inevitably search for one. Each of the party models "has been practiced at some time or another in the course of American elections," but "none has been fully and permanently achieved" (pp. 104, 108). There seems, however, to be no developmental pattern or progression, nor any reason why one model dominates at any one time. Pomper does plant a major clue: "The character of the American electorate prevents the full realization in practice of any of the party models" (p. 114). Indeed, "although the fit is not precise, American electoral behavior is particularly consonant" with those two models the reader will think of as party government and Downsian choice (p. 114). The suggestion is, I take it, that the parties function as elite-led organizations responding, in their need to win elections, to the whims of voters. The important point is that they respond, apparently, not only with shifting issue positions but by transforming themselves as parties.

Taken as observations on the multifaceted, shifting nature of the American parties, the book is an unqualified success. It is marked, as is all of Pomper's writing about political parties, by both lucidity and involvement. His observations on a wealth of party experiences and a great range of writing about them are the book's great treasure. As an exercise in concept development, however, it leaves the reader somewhat at sea. One might reasonably conclude that the a priori, classificatory style is a somewhat inefficient way of analysis. My own preference in concepts is for a well-focused snapshot, rather than a kaleidoscope.

As the title indicates, Pomper is especially intent on relating parties to democracy. He quotes, approvingly, Schattschneider's famous dictum, "Political parties created democracy and that modern democracy is unthinkable save in terms of the parties"; and he proclaims that his book "celebrates their continued union" (p. 19). His preference among varieties of democracy is for a maximum concept—one this goes far beyond mere process to embrace substantive goals and problem-solving capacity. But while concern for party contributions to this democracy suffuse the entire enterprise, there is little about democracy per se. It remains a book about political parties.

Despite any disclaimers, Pomper's book now joins a growing list of assessments and analyses of the American parties. His is at least the fourth volume in the past seven years to essay the grand overview, and his is the fourth theoretical perspective that comes to the task. Leon Epstein's *Political Parties in the American Mold* surveyed the American parties from an institutionalist perspective that attributed their special character both to American governmental institutions and the American inclination to regulate the parties. Denise Baer and David Bositis' *Elite Cadres and Party Coalitions* brought the unifying insights of elite theory. Joseph Schlesinger's *Political Parties and the Winning of Office* applied his well-developed vision of rational choice in describing parties as office-seeking nuclei. The Pomper perspective, especially with its emphasis on the normative, is clearly a fourth option. (It may also be worth pointing out that some perspectives, such as the fashionable functionalism of a generation ago, are missing in recent summings up.) The multifaceted American parties, thus, have their multifaceted literature; and scholarly consensus about them seems more remote than ever.

Ultimately Pomper's normative and descriptive concerns merge in two final chapters on change and reform—the first discussing major reform movements (e.g., the APSA's report on responsible parties), the second containing Pomper's own prescriptions. In order that we might have parties that are "leadership coalitions with a heightened degree of policy coherence, supported by a more active mass base" (p. 146), Pomper would, inter alia, shift the nomination of party candidates for office back to the party membership, increase private and public funding of the parties qua parties, reduce state regulation of the parties, and extend the social and political role of the parties (e.g., operating cable television networks and offering cooperative purchasing to their members).

Very few political scientists who look at the American political system these days will deny that the proliferation of organized interests and the disaggregation of the American electorate has led to a crisis in governance. Most will agree, too, that strong political parties offer the best way of organizing the majorities for governmental action. Some, like Pomper, remain optimistic and constructive about rebuilding the parties to do so. Others (I am one of them) only wish they could be so optimistic.

University of Minnesota FRANK J. SORAUF

Knowledge, Power, and the Congress. Edited by William H. Robinson and Clay H. Wellborn. Washington: Congressional Quarterly, 1992. 255p. $30.95 cloth, $19.95 paper.

The general theme of this edited volume is the place of knowledge in legislative politics. The 10 papers and 14 commentaries cover a variety of philosophical, historical, and policy aspects of the role of information in legislative decision-making processes. The authors of the papers range from Theodore Lowi (who expands on his argument in *The End of Liberalism*), to Allen Schick (on the place of policy research), to Ernest May (on the increasing importance of knowledge in national security policy). Commentaries are offered by Representatives Thomas S. Foley, Newt Gingrich, David E. Price, and Stephen Solarz, along with a mix of social scientists and historians.

Readers will find little new in a majority of the papers, most of which are quite brief and some of which are only weakly connected to the general theme. One exchange—between historian Gordon Wood and political scientists Joseph Cooper and James Sterling Young—deserves special attention for the importance of the issues raised and scholarship that lies behind the contending positions.

Wood begins by observing that the place of specialized knowledge in legislative affairs was just as much an issue two hundred years ago as it is today. In the first Congress, which was faced with the daunting task of elaborating on the bare framework of the Constitution, "everything seemed new, unfamiliar, unprecedented, and the members struggled to agree on what to do, on what examples to follow, not only in matters of tariff and trade policies but also in what seems to us to be elementary matters of rules and ritual" (p. 49). Consequently, Wood argues, "a premium was put on debates in Congress as a source of information and shared wisdom" (p. 50). Information and expertise, what little there was, were highly valued. Indeed, congressional dependence upon executive agencies for information and legislative drafts quickly became an issue. And as it did, even more basic questions—the nature of representation in Congress and the kind of knowledge that representatives brought to their deliberations—were raised, questions left unresolved by the ratification debate.

The classical republican vision of the Federalists, who sought representation by virtuous men capable of enlightened outlook, triumphed in the ratification debate. Yet, Wood argues, the pluralist vision of the Anti-Federalists, who insisted that self-interest, not special knowledge, should prevail in practice, soon became the dominant view of American politics. Wood's concise analysis of the two visions leads him to a conclusion about the place of knowledge and self-interest in politics that is probably shared by most political scientists.

Wood is challenged on two fronts. Cooper argues that the Anti-Federalist vision is both unrealistic and inadequate as a standard for evaluating American democracy. Young asserts that the founders and early legislators knew a great deal about governing. Cooper takes exception to the standard view of what happened to American politics. He begins by agreeing with Wood that the Federalists and Anti-Federalists present different views on the role of reason and interests in politics, the nature of representative government, and political leadership. But Cooper argues that Wood overlooks evidence on three counts: (1) political outcomes are the product of much more than self-interest and bargaining: they often turn on shared values, rational argument, and (occasionally) the public interest; (2) the institutions designed by the Federalists worked largely as intended—that is, on occasion, politicians have risen above mere interest-group politics to offer new policies that reflected a broad public philosophy; and (3) the Anti-Federalists offered no adequate prescription for the design of political institutions. Cooper therefore insists that we hold on to the Federalist vision for guidance about how the American system should and does work.

Young claims that although the members of the first Congress may have been novices at governing a nation, they, remarkably, did not perform like amateurs. On the contrary, from their years of service in colonial and state legislatures they had absorbed two critical lessons. First, they knew how to exercise power without processing executive authority. Colonial assemblies, Young argues, were schools where Americans learned to represent home constituencies and to lobby and even persuade administrative superintendents. Second, they knew how to make policy consensually, rather than hierarchically. Deep divisions within the assemblies had to be resolved if the assemblies were to have any influence at all in the context of colonial governance. As Young summarizes it, "The way of governing the Constitution prescribed—government by a set of separate institutions with no one on top—was consistent with the way that Americans invented long before they had a nation to govern" (p. 78). Thus, the members of the first Congress were information-poor by modern standards; but critical knowledge was readily at hand.

Not all contributions to this volume are up to the standards set by Wood, Cooper, and Young; but nearly all provide insights that make this collection worth reviewing by all serious students of congressional politics.

University of Minnesota STEVEN S. SMITH

Linking Citizens to Government: Interest Group Politics at Common Cause. By Lawrence S. Rothenberg. New York: Cambridge University Press, 1992. 306p. $59.95 cloth, $18.95 paper.

While an impressive array of both theoretical and empirical information has been accumulated dealing with such concerns as the collective action problem or the activities and impact of political action committees, research in the field of interest group politics is fragmented, and a number of important areas have been conspicuously neglected. The relative lack of comprehensive, theoretically based studies of individual organized interests represents one such research gap. Factors such as the discipline's generally low regard for case studies, the intimidation that researchers may feel in approaching private and semiprivate organizations sensitive about their visibility and often reluctant to grant the access necessary for serious scholarly work, and the financial and other research costs faced by researchers seeking to gather hard data about an interest group are among the factors that have acted as barriers to the in-depth study of organized interests.

From the perspective of the existing literature, Larry Rothenberg's case study of Common Cause is an impressive achievement, and his volume should be viewed as complementary, rather than as a competitor to an earlier volume on the same organization, Andrew McFarland's *Common Cause: Lobbying in the Public Interest* (1984), which is more qualitative in its methodology. Rothenberg attempts to develop and apply to Common Cause what he calls an "integrated perspective on organizations" (p. 5), examining, in the process, a range of questions of interest to those who study political organizations, including why and under what conditions individuals join—and choose to remain with or leave—the group; the background and attitudinal differences among activists, rank-and-file and the leadership of the organization; how political goals are set within the organization; and the impact of the organization in the policy process (focusing on the MX missile decision and campaign-finance reform issues). Finally, he attempts to link demand and supply concerns, relating a number of membership representational features to the group's activities and impact in the policy process.

Rothenberg is able to paint a comprehensive picture of Common Cause, largely because of his ability to make creative use of a variety of hard data sources in his analysis, including the 1980 National Election Study; Constance Cook's 1982 survey of four public interest groups, including Common Cause (made available through the Inter-University Consortium for Political and Social Research); and some of the General Social Surveys. But his main data source is a mail survey administered by a graduate school colleague, Jon Siegel, to twelve hundred Common Cause members in 1982. These data are supplemented by additional information gathered by the author from a number of Common Cause employees, leaders of other public interest groups, and Washington-based legislative staffers and political consultants.

The volume's contribution is both theoretical and empirical. On the demand side, Rothenberg puts forth an incentive theory model—based on imperfect information—that goes well beyond the static models that tend to characterize much of the collective action literature that comes out of the utilitarian economic tradition. Rothenberg adds a dynamic learning component to the decision to join and remain in a political group that is intuitively appealing. Realistically, individuals often have little information about the costs of group membership (other than dues); and most are unaware of many of the potential benefits of group involvement. Rothenberg hypotheses that once they have joined a group, however, they engage in an "experiential search" and then "reevaluate their decision as they learn about the costs and benefits of participating" (pp. 21, 24).

Applying this model to retention decisions among Common Cause members who responded to the mail survey, he finds that members do indeed learn, many tending to drop out of the organization if they had made a mistake, while others may go beyond mere membership to become organizational activists. The author's findings suggest that entrance motivation and motivations to remain are quite different. Rather uninformed individuals often join for vague collective and purposive reasons and later decide either to remain or to exit when specific solidary and selective reasons are crucial. As is typically the case when economic models underlie survey-based attempts to assess why people join groups, there are no really startling findings from his data analysis (people join because they agree with the goals of the group), although some readers may be surprised to find out how important political ambition and career motivations are to many Common Cause members.

The author's examination of internal decision making within Common Cause will be of most value to those concerned with the representative nature of public interest groups—groups often accused of being staff-dominated and rather exploitive of their contributing membership base. He finds that decision making in Common Cause takes on a rather curious democratic form. While the leadership does possess an impressive amount of discretionary authority, the activists in the organization have substantial agenda-setting power; and even the rank and file, whose contributions are needed and who must not be alienated, do retain certain veto powers. Rothenberg's mini–case study of how Common Cause came to invest organizational resources into trying to stop the MX Missile—representing a major change in the group's policy orientation—makes for the most interesting and insightful reading in the volume.

The author does less well when he turns to assessing Common Cause's impact in the policy process. His attempt to use a probit model to assess the distinct impact of anti-MX missile lobbying and pro-campaign-finance-reform lobbying upon a number of legislative roll-call votes is not very convincing, given the difficulty of separating Common Cause lobbying influence from other lobbying efforts and the impact of other groups on such issues. All Rothenberg really does here is to demonstrate that Common Cause was highly involved and active on a number of key political issues—demonstrating that the group's impact simply cannot be gauged from his analysis in any precise way.

Still, this is an important book that should be read by interest group scholars and those interested in general representation matters in American politics. As with all case studies (even those as well done as this one), generalization is tenuous. Not only is Common Cause atypical as an organization even among public interest groups, but it is constantly changing, as well. What Rothenberg has provided is a way to think about and conduct research on individual groups as integrated entities. It is a large step in the right direction.

University of Kansas Allan J. Cigler

COMPARATIVE POLITICS

History and Context in Comparative Public Policy. Edited by Douglas E. Ashford. Pittsburgh: University of Pittsburgh Press, 1992. 365p. $44.95.

More than anything, what is manifested here is the difference between a collection of lovely essays and a lovely collection of essays. These 15 essays (a third from the editor's own pen) are grouped loosely into three sections: "In Search of Time," "In Search of Context" and "In Search of Agency." The aspiration is to produce a postpositivist—if not quite postmodernist—style of policy analysis, one more historically and cross-culturally sensitive than is traditional among certain sorts of data-driven students of comparative public policy.

Exercises in large-scale, many-nation, many-period number crunching have, indeed, fallen into disrepute. This is true not least for the reasons well captured in Ashford's telling anecdote about the "analyst of French communal budgets [who], laboring to extend a data bank to 1871, was mystified until someone told him of the Franco-Prussian War" (p. 27). Or, for another example more compelling than any of those in the relatively weak "Context" section, consider the 1951 Indian Census. Drawing on the neoclassical political economy of Ricardo and Smith, it included a category of "persons who cultivate land owned by another"; but in the Indian agricultural context, this construct nonsensically conflates the least secure peasant producers with the most powerful managers of commercial agribusinesses (see Barry Hindess, *The Use of Official Statistics in Sociology* [1973], 29–34). In doing policy studies, it is undeniably helpful to know a fair bit about the background of any statistics you find yourself coding and analyzing.

In his several chapters, Ashford himself makes that methodological point effectively, drawing on eclectic reading and sometimes telling examples. But often even in Ashford's own chapters, and more often elsewhere (as in otherwise excellent overviews of political parties by Bert Rockman and of public bureaucracies by Guy Peters), one gets the sense of a literature review but loosely connected with the central themes of the book. The contents list itself betrays a similar failure of focus. I doubt that anyone setting out to illustrate these basic methodological points would concoct a wish list that necessarily included chapters, however excellent of their kind, on debates among historians on the role of ideas in the growth of government (Krishan Kumar) or Popper's notions of social engineering (James Farr) or, indeed, what we might still learn from functionalism (Norman Furniss). In short, the book adds up to substantially less than the sum of its parts.

The basic problem is too much precept and too little sustained example of what policy analysis—or, indeed, political analysis more generally—in this mode might actually look like. What the book needed was more in the mode of Bo Rothstein's historically and sociologically sensitive analysis of connections between working-class strength and the development of the welfare state in Sweden or Hans Blomkvist's analysis of how Indian experience suggests a whole new model of a "soft" state that is particularistic in all sorts of patron–client ways, rather than universalistic in good Weberian fashion.

Australian National University ROBERT E. GOODIN

Regulating Privacy: Data Protection and Public Policy in Europe and the United States. By Colin J. Bennett. Ithaca: Cornell University Press, 1992. 263p. $39.95 cloth, $16.95 paper.

Colin Bennett's *Regulating Privacy* is really four books in one. First, it tells the story of a substantive issue—individual privacy rights in the age of information technology—and how that issue came to be addressed in the policymaking processes of four nations: Sweden, West Germany, the United Kingdom, and the United States. Second, it examines public policymaking at two levels: the adoption of broad public policies for ensuring personal privacy from computerized recordkeeping and the selection of specific mechanisms to implement those policies. Third, it analyzes policy innovation and diffusion. Finally, it analyzes policy conversion and divergence in a cross-national setting with a particular focus on the issue of state sovereignty. In each case, the book succeeds admirably.

The author chose an appropriate and (as demonstrated by the work itself) effective methodology. He calls it the *comparable cases* strategy, which he describes as the use of "a number of comparable cases . . . in order to introduce an element of *control*" (emphasis original). This strategy enables the author "to explain observed variations or nonvariations of policy content and policy instruments in their transnational and domestic contexts" (pp. 8–9). The research included examination of a number of data sources, including official and "less-than-official" documents, the writings of policymakers and scholars, and extensive interviews.

Bennett begins his analysis with a discussion of the origin and nature of the issue of data protection, which he concludes is "not one problem, but several" (p. 12). The essence of the issue, however, is to be found in the notion of individual privacy rights. These rights and the way they are addressed in public discourse have, in Bennett's mind, three dimensions: humanistic, political and instrumental.

The *humanistic* dimension concerns protection of "the dignity, individuality, integrity or private personality of each one of us" (p. 23). Bennett observes that at the humanistic level, the issue of data protection was virtually identical in all four countries studied. "On this basic humanistic level, it is impossible to distinguish any significant cross-national differences in either definition or emphasis. However the concern is formulated, information technology accentuates the dehumanizing and alienating aspects of modern mass society and information technology" (p. 27).

The *political* dimension concerns fears that information technology to collect, store, and manipulate personal data could become the tool of a tyrannical state. Privacy—at least in the nations examined—is thus seen an essential condition for the maintenance of pluralistic democracy. Finally, the instrumental dimension shows how data protection can serve to advance other values. This mainly consists of ensuring that only "appropriate" data about individuals are collected and kept by governments (and private interests); that these data are complete, relevant to legitimate purposes, and accurate; and that they are not transmitted to others without good reason.

Bennett not only found remarkable similarities among the three dimensions of the data protection issue in Sweden, West Germany, the United Kingdom, and the United States. He also found strong reservations about how information technology had been—and might be—used to the detriment of individual privacy uniformly expressed in public opinion in these nations.

Two factors accounted for the issue of data protection arising on the public agendas of these nations. The first was the rapid diffusion of powerful, accessible information technology. Computers are everywhere, and their power and sophistication is growing dramatically. The second factor was bureaucratization in the postindustrial state. In order to make and implement public policy, governments need data. Bureaucracies are the greatest collectors, manipulators, keepers, and users of data. The presumed rights of individuals to privacy inevitably clash with the legitimate needs (to say nothing of the excessive demands) of governmental agencies for data about individuals.

Bennett argues that the movement for data protection was similar in all four countries. While for all but Sweden, a high degree of cross-fertilization was possible in the development of data protection statutes, each country developed its own solution. Yet the statutes providing for data protection that resulted were more notable for their similarities than for their differences. In adopting data protection legislation, all but Britain were motivated by national concerns. The Conservative government finally adopted a data protection stature out of concern that without one, the British information technology industry would suffer economic harm.

However, the mechanisms that the nations chose to implement their data protection policies were quite different, reflecting different national political cultures and styles. Bennett's discussion of the reasons the nations chose different implementation mechanisms and his examination of policy convergence and implementation divergence—and policy innovation and innovation diffusion—are among the strongest portions of a generally strong book. His analysis is balanced, and his conclusions are convincing and flow directly from the data.

It is unusual to read a book that does not have at least one or two notable weaknesses. Whatever the weaknesses and limitations of *Regulating Privacy*, they are hardly important beside its strengths. This is one of those rare academic books that the more I read, the more I liked. It is well written (Bennett not only knows how to use the language well, he also knows how to tell a good story), informative, tightly reasoned, and analytically rigorous.

It should be read by scholars in the policy disciplines and by those interested in privacy rights, innovation and diffusion, and cross-national political analysis. They will not only learn about an important issue and how it was handled by the policy processes of four nations. If they read carefully, they can learn a great deal about doing solid research, thinking clearly, and writing well.

In my opinion, *Regulating Privacy* is an excellent book, and I look forward to reading more from this young scholar as his career progresses.

University of Maryland, Baltimore County DONALD F. NORRIS

How Policies Change: The Japanese Government and the Aging Society. By John Creighton Campbell. Princeton: Princeton University Press, 1992. 418p. $39.50.

As the title and subtitle suggest, Campbell's densely written book pursues several goals. Why the elderly have been dealt different hands from other "policy takers" in Japan constitutes the basic research framework. But while mapping that archipelago, he was overtaken by the notion that his scientific mission bore a parallel to that which carried Darwin on his famous journey to the Galapagos Islands. While the hegira of the wizened Orientals might not reveal the master clue to societal evolution, it just might hold the key to a more general theory of policy change.

His imagination was triggered by recognition that the extension of state support for the Japanese elderly was difficult to explain in terms of prevailing Western policy paradigms, be they of the Bismarck or the New Deal variety. Particularly inscrutable were the timing of income support and health care policy changes. Similar problems have been tackled for other Japanese policy areas by Calder in terms of orthodox political concepts, but Campbell felt it necessary to tackle issues on a more general plane.

No slave to pseudobiological paradigms, Campbell turned to physics for inspiration. His scientific mentor was less Bagehot than that other coiner of immortal policy tags, Richard Rose, whose notion of inertia as the dominant explanandum of policy, propelled his theoretical scan. If neither social, nor demographic, nor economic variables would suffice, perhaps the basic concept of energy needed to be employed. Just as subterranean stirrings set the timing of Japanese volcano eruptions, so the propulsion of terrestial "garbage cans" of energy could have triggered the timing through in which policy spigots were turned off and on in the 1970s and 1980s.

Where other welfare state theorists developed explanatory models that relate ideas to demographic, political, and social variables, Campbell presents a more economical model. While not attempting as precise a formula as Einstein, his conceptual formulation is also reducible to just two terms: one for energy and another for ideas. When so many energy-laden "garbage cans" mix with so many idea-laden "heclos," watch out for that unleashed bullet-train, sweeping policy change down the fast track.

When the elderly-arena policy train moves down what is called the artifactual mode, it does so concurrently with other Japanese policy trains on other tracks. In his own survey, the medical policy train, distinguished by organized interests and overt policy rivalry, runs on tracks similar to those taken by health-policy trains in other countries.

The artifactual mode is conceptually anchored in randomness, and hardly any characteristic seems less appropriate to the Japanese modus vivendi. Hence, the notion that a relatively pure specimen of the artifactual type was identified in Nipponese waters is particularly provocative in terms of Westerners' views of Japanese policy culture. But while the notion of Japanese bureaucrats taking random policy walks seems mind-boggling, Campbell's highly detailed and sophisticated analysis encourages us to entertain the notion that beneath all the camouflage, this is basically what occurred. If we can entertain this interpretation, then it is only while noting the unique inability of the Japanese elderly to produce even the tamest of grey tigers.

Turning from the innovative to the mundane aspects of goals, it may be said that Campbell presents a superbly detailed and finely crafted descriptive analysis of several decades of development in the several policy areas bearing on elderly policy. The analysis seems as inclusive as it could possibly get and, for the first time, opens up the internal dynamics of Japanese social policymaking to the non-Japanologist. In its scope, the work goes beyond the framework of masterly American studies like that by Derthick and is perhaps most similar in structure to Nipperdey's comprehensive and data-rich study of post-war German social policy.

It was a feat of great dedication and persistence that produced not only the bold theoretical endeavor but also the detailed analyses of stages of policymaking and -remaking in a way that must elicit the highest respect of the specialist, even while proving accessible to the novice new to the landscape of Japanese social policy.

Washington University ARNOLD J. HEIDENHEIMER

Beyond the Protective State: The Political Economy of Australia's Manufacturing Industry Policy. By Ann Capling and Brian Galligan. New York: Cambridge University Press, 1993. 272p. $54.95.

One of the interesting paradoxes of the 1980s is that the proponents of market liberalization had their greatest successes precisely where the institutional strength of the state was strongest, and that only states able to assert their autonomy from social groups could actually subject those groups to market pressures. Autonomy begat autonomy. Australia is no exception, and precisely because it remains terra incognita to most U.S. and continental political science this study is a useful addition to the literature on liberalization in the 1980s. However in order to make its subject accessible to readers unfamiliar with antipodean politics this book sacrifices some depth in favor of simplicity.

Capling and Galligan, in one of a recent series of fruitful collaborations between Canadians and Australians, argue that the Australian state has shifted its industrial policy from protection to correction. Prior to the 1980s, industrial policy concentrated on protecting any and all industry with extremely high tariffs and tight import quotas. This created a wide range of internationally uncompetitive industries. These industries in turn provided employment for many more Australians than would otherwise have been the case; a highly redistributive and egalitarian system of judicially set wages shifted rents away from export industries and towards workers in protected industries. By the 1980s this policy had become untenable, because raw materials exports no longer generated enough rents for redistribution. Unlike the 1930s depression, which saw an intensification of protection, the state reversed course in the 1980s, reducing protection. Instead, industrial policy corrected the negative aspects of the earlier blanket policy by using a targeted industrial policy to foster internationally competitive industries.

The book begins with a survey of theories of the state in the Australian context. This refraction of conventional theories through the Australian lens is both useful and disappointing. Capling and Galligan show how badly off-the-rack American theoretical clothing, particularly pluralism, fit the odd-shaped Australian body politic, concluding that statist theories best explain Australian politics and institutions. Their survey provides a fine introduction to Australian political thought for the non-specialist, and their insistence on the primacy of the state, despite Australia's Westminster heritage and English common law tradition is essentially correct.

But they omit any discussion of the institutional arrangements that constitute and limit state strength in Australia. Collective bargaining institutions and their consequences for unions and employers' organizations are largely ignored, even though these organizations are the major social players in the politics of tariff setting. Similarly Australia's federal system and preferential voting arrangements have helped constitute a potent rural interest group. The state and state policy constructed all these groups, and in each episode of major institutional change in Australia federal ministers and bureaucrats relied on the support of some of these groups to neutralize the opposition of other groups. Tariff policy is no exception to this rule. This omission obscures the institutional sources of Australian state autonomy, particularly for non-specialists.

The middle of the book lays out the institutional history and trajectory of industrial protection. Moderate tariffs created a broad political coalition in favor of even higher tariffs, and the economic shocks of the depression led to extremely high tariffs that persisted into the early 1970s. But under Labor governments in the early 1970s and even more so the 1980s, the state unswaddled protected industry, phasing out tariffs and import quotas, reducing preferential buying practices, and increasing market pressures on firms.

The book ends with three case studies—steel, automobiles, and the textile-clothing-footwear complex—of this unswaddling process. Here the authors show how the state evolved meso-corporatist arrangements to induce and coerce cooperation from producers and the relevant unions. These case studies are not fully representative of trends in Australia, however. Looking at the equally sheltered metal bashing industries, for example, would have shown a more societally based meso-corporatism with unions taking the lead in changing production practices.

This study has two weaknesses. First it focuses too narrowly on the creation and reduction of tariff and non-tariff protection for manufacturing. Capling and Galligan only briefly situate this border protection in the larger context of state regulation of industry. Although they clearly understand that Australia's unusual system of court-set wages and work conditions lies at the core of Australia's political economy, they mention this only in passing. It is possible that the wage arbitration system was and is the biggest piece of on-going industrial policy in Australia because of its homogenization of wages and its limitation on the adoption of alternative work practices. The arbitration system was also reorganized during the 1980s, although not as much as the system of border protection. It would have been more interesting to tie the reorganization of industrial protection to the reorganization of work and wage setting.

Second, and related to this, is the omission of any discussion of state actors' motivations for transforming the protective system. This makes it hard to discern why the state had autonomy and why actors opted for what seemed in prospect very politically unpopular steps. The authors discuss a few key actors' personal preferences, without any reference to the preferences of the social groups these actors represented. All of the key actors in the 1980s were either politicians or union officials (or both), and the authors note that the minister ramrodding liberalization relied rather more on his personal staff than ministry personnel when formulating policy. This weakens the contention that policy emerged from an autonomous state by eliding unions' sometimes diverging preferences for specific policies. I should think that this also makes the trajectory of political struggles somewhat opaque to a non-specialist, particularly in the detailed case studies. This kind of policy shift always produces winners and losers; what were they doing? Equally important, the authors never really demonstrate the state's interest in this policy shift, aside from a few passing references to external economic shocks. Filling in these details, however, would have threatened the authors' contention that the state is the protagonist in the reorganization of Australian political economy during the 1980s by showing that a coalition spanning state and society, and which stands to gain from liberalization, has motivated this and other policy reversals.

Despite these weaknesses the authors do provide the first systematic overview of manufacturing industry policy in Australia during the twentieth century and of the astounding policy reversal that occurred during the 1980s. Specialists will be delighted by the detailed discussions of policy formation in the case studies. Non-specialists stand to learn much from the beginning and middle. Given that liberalization went farther in Australia (and even more so New Zealand) than in many European economies, they might learn something about why policy change did or did not take place 'up over.' They also can learn from the discussion of how a state can construct and use meso-corporatist institutions as an instrument of industrial policy.

University of Virginia HERMAN SCHWARTZ

The Development of Political Science: A Comparative Survey. Edited by David Easton, John G. Gunnell, and Luigi Graziano. New York: Routledge, 1991. 296p. $49.95.

Social Sciences and Modern States: National Experiences and Theoretical Crossroads. Edited by Peter Wagner, Carol Hirschon Weiss, Björn Wittrock, and Hellmut Wollman. New York: Cambridge University Press, 1991. 374p. $49.50.

The doctoral training of most American political scientists attends scarcely at all to the history of the discipline. Older introductory courses on the history, scope, and methods of political science have yielded to more extensive training in research design, statistics, math, and the analysis of arguments. As a result, fewer and fewer political scientists in the United States know much about their predecessors. The aims and understandings, successes and failures, and even the names of these forerunners become lost to a strangely present-minded discipline.

Perhaps the greatest value of these two books is to challenge and repair this proclivity. By surveying various national traditions of political inquiry, both also help to expand the horizons of political scientists whose sense of the discipline suffers confinement to a single country. Like the presentism, this parochialism is mainly an American legacy. Where political sciences have begun or have become Americanized, the practitioners "forget" what has been tried and why—losing the compass that could make it possible to keep their bearings in current inquiries.

Another reason to cultivate historical awareness of the discipline is the inevitable recognition that any history must promote specific, sometimes uncritical, concerns of politics. Seeing the politics in the prior efforts of others, we can become more intelligent about our own. In this respect, the cross-cultural scope of each volume is especially telling, since virtually every essay ends up showing how the politics of particular places and times mark these different political sciences, no matter how carefully "scientific."

A notorious trouble with brief reviews of detailed histories is an inability to do justice to their details. The episodic character of both collections aggravates that difficulty, since the precise focus of any one chapter typically differs considerably from the ones before and after. Overall, though, *The Development of Political Science* tends to emphasize differences, during the past century or so, in the sources and trajectories of national traditions of political science. Even though it focuses on America and Europe (with glances at China and Anglophone Africa), the book leaves a general impression of diversity in the goals, questions, and organizations of "the discipline." The exploration of ties in *Social Sciences and Modern States*, by contrast, suggests that the local politics and practices of policy sciences reveal, in their many variants several, overarching (but not overriding) dynamics in the development and decline of "the welfare state."

John G. Gunnell begins the essays in *The Development of Political Science* by showing how "accounts of the past of political science have heretofore been primarily instruments of disciplinary legitimation and critique." Presumably, the rest of the collection attempts to make good on Gunnell's further argument that disciplinary histories "must now, even to serve these functions successfully, develop a more adequate 'historical sense' and confront issues of historical validity" (p. 13). Subsequent chapters explore such historical vagaries as the historical, philosophical, and economic biases identified by Jean Leca in French political science (pp. 164–75). Elsewhere, as well, these sorts of factors explain both "the muted impact of American political science" (p. 96) found by Jack Hayward in Great Britain and the nation building sought through "the Americanization of African political science" (pp. 254–55) described by L. Adèle Jinadu. The template for most of these stories is the liberal democratic fear that science too easily and often becomes an instrument manipulated by politics. The popular democratic fear, more prominent, these days, in the humanities, is that scientific expertise will work to eliminate the political choices and voices of citizens. Together, these form the Scylla and Charybdis of our late modern sciences of politics.

The largely historical essays in *Social Sciences and Modern States* also tend to confirm how political the "policy sciences" have been. Some chapters make this a test of disciplinary effectiveness; others treat it as evidence of disciplinary complicity in the career of the late modern welfare state. Thus, the pieces in part 2 (by Martin Rein and Donald Schon, Giandomenico Majone, Carol Hirschon Weiss, and Björn Wittrock) are especially intriguing for their ideas on modes of policy science that might be suited to politics beyond the welfare state. Weiss is particularly good on the political contexts and interests that generated the social sciences as we now know and practice them.

Either way, however, both volumes leave us crushed in an impossibly straight and narrow way between the Scylla of politicized science and the Charybdis of authoritarian politics. Given the contributions from diverse cultures, it is odd that neither volume questions this premise. Virtually all the essays privilege the honorific and rather antipolitical sense of "science" pioneered in the Anglophone world to demarcate "real science" from any other inquiry (no matter how systematic) that lacked the present list of approved methods. Though Gunnell's chapter calls attention to the cultural and historical variability of "science," the rest of *The Development of Political Science* offer internalist accounts that presume that we know in each case, from the outset, what to count as "political science" and why. *Social Sciences and Modern States* is more critical about what to regard as "policy science"; but even there, the exclusions are surprising: Why examine such disciplines as economics, sociology, and political science while omitting geography or urban and regional planning?

Nevertheless, both books contribute to a more critical and adequate sense of current practices in political science. To have our attention drawn back belatedly to the likes of Lasswell, Lerner, and Merton is to be reminded of "policy scientists" who addressed themselves insistently and critically to the diverse uses of political concepts and assumptions still prevalent in the discipline's work. Many of these concepts and assumptions, in fact, now trouble our studies by staying beneath notice (let alone beyond contest). Earlier figures and debates in the discipline show how problematical our talk of "representation" and "accountability" can be for the sprawling, mass-mediated polities with shared economies and ecologies that we now call our own. Likewise, to compare with David Easton and company the institutional and methodological developments of "political science" across political boundaries is to be warned against the simple dichotomies between "science" and "politics" that continue to inform our work.

Both books have a great deal to teach us, then, if not always by advertence. They should prove especially valuable for scholars fairly new to the discipline of political science. And since both volumes represent continuing series of conferences and publications devoted to better appreciation of the discipline's development and discourses, we have reason to hope that in coming years, their beginnings will be enlarged with increasing historical skill and political sophistication.

University of Iowa JOHN S. NELSON

Predatory Rule: State and Civil Society in Africa. By Robert Fatton, Jr. Boulder: Lynne Rienner, 1992. 165p. $30.00.

Robert Fatton's coverage of the specificities of politics and class in Africa as well as the review and evaluation of the origins, depth, and implications of Africa's deepening crisis, provides a very refreshing departure from the more mainstream, even "radical" works which have come to dominate the field of African studies. As well, *Predatory Rule* moves away from the revisionist euphoria which seized African Studies with the disintegration of the Soviet Union as a nation and superpower. Rather than accede to Francis Fukuyama's position on the "end of history," Fatton demonstrates that if anything, history is just being made in Africa. He does this by drawing a new map for understanding the vicissitudes and predictable undercurrents of politics and society in Africa, concretely locating his analysis in a dialectical presentation of class relations and class politics a la Antonio Gramsci, and recognizing the interrelatedness of culture, power, production, exchange and consumption relations in contemporary Africa.

Fatton's book contains seven chapters including the introduction and conclusion. In the introduction, the author sets out the theoretical framework of the book and quickly concedes that his effort is in no way an attempt to "construct . . . a grand theory of African politics" (p. 1). He goes on to argue that the "conceptual map of African Politics is dominated by three fundamental sites: ruling class formation, class disarticulation, and subordinate class resistance" (p. 2). These "sites" are effectively discussed in their specific and interconnected dimensions within the overall theoretical construct of hegemony and civil society. Ruling classes are presented as predatory and in search of hegemony over subaltern classes; associational life is presented as having no "predetermined destination"; and civil society is presented as "potentially, a highly subversive space, a space where new structures and norms may take hold to challenge the existing state order" (p. 6). In the second chapter of the book, the author examines the relationship between the three sites, the state, and hegemony. Disagreeing with the earlier formulations of Theda Skocpol and Eric Nordlinger that the state is capable of frontally confronting or challenging the critical interests of the dominant classes, Fatton contends that "over the long term the state constitutes and is bound to constitute the ultimate organizer and defender of the long-term interests of the ruling class" (p. 19). The third chapter deals with the "contradictions of presidential monarchism in Africa." Here the author, taking a cue from Richard Sklar's 1983 address to the African Studies Association, demonstrates that in several respects the inability of Africa's ruling class to become hegemonic, the irresponsibility of the leaderships, widespread repression, corruption, and gross economic mismanagement have accentuated the rapid "descent into hell and squalor" (p. 41). In chapter four, the author deals with the emergence of civil society in Africa. Defining civil society as the "aggregate of private forces engaged in nonstate activities standing paradoxically in the way of the state," Fatton moves some distance away from the on-going praise-singing and undiluted acceptance of all so-called Non-Governmental Organizations (NGOs), pro-democracy movements and associations in Africa. He notes that civil society is "not a tabula rasa; it is conditioned by the political and cultural processes defining the state domain" (p. 75). Fatton warns that many of the so-called associations in civil society might just be opportunistic movements and, like churches, are likely to contribute "more to accepting subordination than challenging it" (p. 78). In chapter five, Fatton examines the democratization processes in Africa and warns of the continuing patterns of corruption, suffocation of civil society, and superficial changes in antidemocratic cultures in African states. Chapter six examines the depth of the African crisis, the content and impact of imposed orthodox IMF/World Bank structural adjustment programs, and the deepening crisis and marginalization of the region in the global division of labor. Unlike the majority of recent works on the African crisis, Fatton acknowledges the historical origins of the crisis and the other "multifold" dimensions of the African predicament. He discusses in detail the alternative program of the Economic Commission for Africa (ECA) and the 1989 World Bank response to the crisis and the new emphasis on NGOs. The author warns against an exaggerated importance and power of NGOs in the face of monumental and multifaceted crisis confronting Africa. In the final chapter, Fatton takes issue with advocates of "governance" who overlook the "material realities, class interests, and predatory relations nurturing the structures of power, affluence, and status" in Africa (p. 142). He concludes that it will not be easy to unseat the predators in Africa because the "site of subordinate class resistance is still too weak to alter the existing structures of power and privilege in a fundamental way. It is also severely affected by the reactionary impact of the site of class disarticulation. The ethnic, patriarchal, and particularistic appeals of the site of class disarticulation blunt the revolutionary potential of subordinate classes" (p. 146).

Without doubt, Fatton's book is a major intervention in the almost boring and stalemated debate on the politics of power and class in Africa. It is very rich in facts and concrete examples. Fatton goes beyond description, personalities, and the usually more simplistic conceptualization and interpretation of African politics, to demonstrate the need to return to history, class, social relations, production patterns, and specificities in order to achieve a holistic and rational appreciation of the fine points of African politics. His analysis of the military, ethnicity, civil society, corruption, structural adjustment, NGOs, the church, pro-democracy movements and so on, reflects an up-to-date understanding of the finer points and "inner workings" of the real stuff that make up politics in Africa. As well, his positions on the democratization process and criticisms of the simplistic advocacy of "governance" as a way out for Africa, draw attention to the real fact that "governance" in itself cannot exist or operate outside the objective material conditions which determine and condition the nature of politics, power and class.

Some of Fatton's positions, for example, his insistence on the analysis of class and material conditions and on a revolutionary resolution of the crisis, might be received with reservation by the new revisionists. The reality is that social science discourse which arose in the more developed social formations following the developments in Eastern Europe and the global triumph of the market, does not and cannot fully explain the political and economic realities of Africa. As Fatton rightly demonstrates in several portions of the book, the real stuff of African politics is conditioned by the realities of Africa's history, its specificities, and the dynamics of politics.

Predatory Rule is a must for all students and teachers of African politics. While it has not covered some issues and insufficient attention has been paid to gender, the environment, students, and even the revolutionary potentials of factions/fractions of the military, Fatton has produced an easily readable, historically accurate, major challenge to opportunistic and simplistic interpretation of politics and society in Africa.

University of Texas, Austin JULIUS O. IHONVBERE

Peru's APRA: Parties, Politics, and the Elusive Quest for Democracy. By Carol Graham. Boulder: Lynne Rienner, 1992. 267p. $37.50.

On April 5, 1992, Peru's eleven-year-old democracy was shattered by President Alberto Fujimori. Fujimori, who had been duly elected twenty months earlier, suspended the 1979 constitution, arrested several opposition leaders, padlocked the congress, and dismantled the judiciary. The *autogolpe* (coup by the president's own hand) was supported not only by most military officials and business elites but also—and more unusually in the Latin American context—by a large majority of the Peruvian people.

To understand the disillusionment of both Peru's elites and popular sectors with the country's constitutional democracy, Carol Graham's book is invaluable. Graham focuses primarily upon the APRA party during its five years in power under President Alan García, between 1985 and 1990. The core of Graham's argument is that, while the external challenges facing the García government were severe, the shortcomings of the APRA party itself were critical to its failure.

The key original empirical evidence behind Graham's argument is presented in her chapter on "APRA and the Marginalized." She examines the operation of an emergency employment program called the Programa de Apoyo de Ingreso Temporal (PAIT) in Huascar, a sector of a Lima shantytown; employing elite interviews and carrying out a small survey of Huascar residents who worked in the PAIT, she assesses the program critically. While Graham acknowledges that the PAIT provided incomes to needy families and also enabled improvements to community infrastructure, she points out that the program was for practical purposes terminated after little more than two years in operation: "The program raised expectations and caused disruption in some communities and then was ended without warning" (p. 196). In particular, some of the women who went to work for the PAIT withdrew from the "Communal Kitchen" and "Glass of Milk" programs in which they had been participating; in other words, the PAIT disrupted existing programs that were to endure (and become even more important as a means to subsistence in the shantytowns) after the PAIT ended.

Moreover, while only one member of Graham's random sample of 23 PAIT workers was a member of the APRA party, party membership was necessary to be promoted to leadership ranks. Graham concludes that, overall in its activities in Huascar, "party favoritism was pervasive" (p. 198). APRA's clientelist politics undermined its "role in implementing comprehensive reform and consolidating democracy . . . " (p. 199).

The analysis advanced by Graham is cogent and sophisticated, and her critical perspective is shared by most Peruvians. However, some additional research would have been advantageous. A larger survey, encompassing more respondents and more attitudinal items about the APRA party and democracy, would have been helpful. Some consideration of the government's "regionalization" policy, which devolved a measure of power to newly created regions, would have been appropriate. Also, Graham frequently compares the APRA negatively with the Venezuelan reformist party Acción Democrática (AD), but does not present convincing evidence about the AD's eschewal of clientelism.

Graham's study also raises some important analytical questions. Graham is in good company among scholars of Peru when she criticizes President García's autocratic style. But, despite political scientists' frequent use of the term "authoritarian," have we really developed indicators for this concept so that we could validly compare a set of leaders on this criterion? For example, while Graham (with good reason) points to García's willful attempt to nationalize ten Peruvian banks as evidence of his authoritarianism, García's takeover bid was ultimately foiled (as she could indicate more fully). Also, García was one of the few Latin American leaders of the 1980s to be elected on a leftist or center-leftist platform who did not shift dramatically towards the right after his inauguration; is a president's commitment to his campaign promises an indicator of his commitment to democracy? While Graham suggests that García was as autocratic as many leaders of non-constitutional regimes, the indicators that would allow us to make this judgment are not clear.

Overall, however, Graham's study greatly enhances our understanding of the failure of the 1985–1990 APRA government. While Crabtree's *Peru under García* (1992) is more helpful in comprehending García's economic policy and the international economic reaction, Graham's argument is essential to understanding Peruvians' current disillusionment with the political parties of the 1980s. As Graham correctly feared, the delegitmization of democratic institutions under the APRA government was a key factor in the breakdown of Peru's democracy in 1992.

George Washington University CYNTHIA MCCLINTOCK

Privatization and Liberalization in the Middle East. Edited by Iliya Harik and Denis J. Sullivan. Bloomington: Indiana University Press, 1992. 242p. $35.00 cloth, $14.95, paper.

This collection of essays is a timely assessment of the economic reform process undertaken in most countries of the Middle East in the 1980s. While displaying a diversity of theoretical perspectives, it offers a handy summary of reforms, country by country, throughout virtually the entire region, excepting Israel and Lebanon, that had once belonged to the Ottoman Empire. The only other major Middle Eastern countries not included are Iran and Morocco. Egypt gets the most attention, with four articles, covering more than one-third of the book; but Fred Lawson and Kiren Chaudhry shed more light on the less-well-known cases of Syria, Iraq, and Saudi Arabia in their respective comparative studies. While their essays are the most original and theoretically stimulating, the other contributions are also of high quality.

Though the theme of "privatization and liberalization" is awkward, the buzzword of *privatization* so dear to neoliberal policy-makers could not be avoided. In his introductory essay, Iliya Harik explains that privatization was taken by most of the contributors in the narrow sense to mean "transfer by the state of public-sector enterprises to the private sector and/or the liquidation of enterprises," rather than, "in a broader sense, to mean the *liberalization* of the economy in such a way as to expand the economic freedom of the private sector" (p. 1). Otherwise, the title of the book would be redundant. Kiren Chaudhry had the pithiest statement of the theme: "Liberalization essentially means allowing prices to rise by curtailing state subsidies; privatization involves divestiture of state assets" (pp. 151–52). But the articles show that while inflation may be rampant in the region, there has been precious little privatization in the narrow sense. Tunisia and, ironically, Iraq lead the way; but even Turkey, the model International Monetary Fund/World Bank pupil of structural adjustment until 1987 or so, divested very little of its portfolio of state economic enterprises. They still accounted for 40% of Turkey's industrial production and 60% of its fixed assets (p. 115); and Marcie Patton indicates that the privatization program stalled in the late 1980s.

Elsewhere, privatization was pretty much a nonstarter. Hence, most of the essays focus on economic liberalization; but this concept, too, is fraught with ambiguity. Harik defines it denotatively to include "relaxation of economic controls and encouragement of the private sector . . . the retreat of the patron state" (p. 1) but then sensibly discusses a wide variety of ownership rights leading to the conclusion that "the debate regarding the public and private sectors may be off the track," since the real issue concerns "the rights to which each is entitled"—by the state (p. 8). Yet his denotations of liberalization do not escape the dichotomy (connoted by privatization) of a public, as opposed to a private, sector. By defining liberalization as changes in state regulations that benefit the private sector, he is still left with a "'green line' separating state and society" even while admitting, "There is simply no such thing" (p. 6). Maybe much of what passes for "economic liberalization" is simply the integration of domestic Middle Eastern markets into international ones—the idea that underlies Chaudhry's cryptic denotation.

Perhaps readers tired of the tales of Islamic resurgence associated with many fashionable books on the Middle East will be grateful for this book's virtual silence on the subject. Only Denis Sullivan (in his discussion of private voluntary organizations) and Abdel Monem Said Aly allude to an Islamic economy; but the latter's concluding speculations about the "Islamization of the private sector" might have been a more fruitful focus than liberalization in the abstract. Most of the contributions either skirt politics or tend, with the blatant exception of Kiren Chaudhry (p. 163), to agree with Harik that economic and political liberalization are mutually reenforcing. But Fred Lawson's comparisons of Syria to Iraq point to major impacts of political Islam on the reform strategies of most of the states in the region.

Despite their common Baathist ideology, the Syrians engaged only in cosmetic reform of their public sector, while the Iraqis went ahead with massive deregulation and privatization in the late 1980s, save in high-tech areas related to Iraq's ambitious military goals. Lawson's explanation, in a nutshell, is that privatization benefited the Iraqi regime's friends, whereas a similar set of reforms in Syria would have enhanced the clout of the regime's Islamist opposition embedded in the dynamic private manufacturing and trade sectors. Comparable forces did not exist or had been eliminated by the Iraqi regime ("so adept at testing the fragilities of the human body," p. 162). Labor, a major source of support for the beleaguered Syrian regime, was no longer a political force in Iraq.

Turning Lawson's argument around, there are affinities—not only in Syria and Iran but also in Egypt and possibly Algeria, Jordan, and Tunisia, as well—between a zealous Islamism and popular capitalism that state-centered, neoliberal policy-oriented approaches to economic reform may miss. Few of the contributors tried to relate their case studies of economic liberalization to broader patterns of political and cultural change. Dirk Vandewalle's description of economic reform in Algeria, for instance, would have made more political sense if placed in the context of the rise of the Islamic Salvation Front, for the reformers and the Islamists worked in parallel, and the Front's economic program attacked Algeria's administered markets (including foreign exchange) in the name of free market principles. Any major structural reform, to endure, will require new political constituencies that only the Islamists seem capable of providing. Algerian reform stalled, like Syria's, after 1990—for the same reason.

Jordan may prove exceptional in being able to integrate Islamists simultaneously into political and economic transitions, but Laurie Brand's careful study of the parallel economic and political processes in Jordan unfortunately predated the more significant changes following the 1989 elections. In the rest of the region, there will be little hope for economic liberalization until policy-makers understand that Islamism has replaced traditional nationalism, socialism, and Arabism as the dominant discourse and that "privatization and liberalization"—however defined—will entail a significant Islamization of the private sector, as already suggested by Abdel Monem Said Aly.

The principal obstacle to reform in the region is the inability of most of the regimes to find other means of accommodating their Islamist oppositions than to brutally repress them as in Algeria, Egypt, Iraq, Syria, and Tunisia. Other obstacles are also worth noting. Marsha Pripstein Posusney gave an excellent account of Egyptian labor's impact on economic policy, complementing Lawson's on Syria and Iraq. In her study of Iraq and Saudi Arabia Kiren Chaudhry pinpointed the principal obstacle to reform: the private sector beneficiaries of the regime are determined to keep their privileges. The most exemplary capitalists of the region may be the Iraqi cronies set up by the regime as a lightning rod for popular outbursts. When "liberalization," as in Iraq and Saudi Arabia (and Turkey for that matter) simply means doling out more subsidies to the wealthiest private-sector groups, is the original definition still consonant with generally accepted meanings?

University of Texas, Austin CLEMENT HENRY MOORE

The Challenge of Restructuring: North American Labor Movements Respond. Edited by Jane Jenson and Rianne Mahon. Philadelphia: Temple University Press, 1993. 440p. $49.95.

This is a superb collection of essays, far better than the usual conference volume. The 19 essays in this book are crisp and concise and descriptive in parallel and complementary ways, and most of them make controversial yet persuasive arguments that point in similar directions. Taken together, these essays offer not only important historical and theoretical argumentation based on comparative analysis, but a much needed vision for potentially resurgent trade unionism. Credit for the clarity and coherence of this volume belongs, no doubt, to the editors, Jane Jenson and Rianne Mahon, who must have worked long and hard to whip the contributions of an impressive set of authors into a series of concise, highly readable essays.

The book is set up nicely in the first two chapters—one by the editors, the other by Ian Robinson. The central framework is a comparative analysis of union response and relative success in the United States and Canada in the face of major industrial restructuring since the mid-1970s. The general argument is that unions have fared better in Canada because "social unionism"—itself a product of historical–political development—is more effective in meeting contemporary challenges to union influence than the more narrow "economistic unionism" of the United States. Yet the implications of this argument move well beyond the more static, bipolar analyses of most past United States–Canada labor comparisons. Canadian social unionism itself may follow the American unions into prolonged decline unless it moves beyond defense of established rights to articulate a realistic vision of the future workplace and national economy in an increasingly global and competitive world economy. And this is precisely what American labor needs, as well, if it is to have any chance of reversing its own decline—perhaps by meeting Canadian labor halfway.

This book makes three main contributions. First, it pulls together a number of persuasive, updated arguments on a question that has intrigued numerous social scientists (e.g., Seymour Martin Lipset, Roy Adams, Noah Meltz, Peter Bruce): In a comparison of "most similar" cases (similar political economies, similar labor movements), why has the fate of American and Canadian labor unions diverged so widely in the past 20 years? Although the arguments of the authors in this volume vary, the weight of the evidence and analysis in the comparative essays by Jenson and Mahon, Robinson, Bruce, Meltz, Joel Rogers, and Donald Wells points clearly to historical, political, and institutional factors, as opposed to cultural or economic ones. And moving beyond the conventional comparisons, this volume adds a third, distinct labor movement—the case of labor in Quebec, richly explored in essays by Gerard Hebert, Brian Tanguay, Celine Saint-Pierre, and Carla Lipsig-Mumme.

The book's second contribution is to set this comparative analysis in the dynamic context of the central economic task of our era—industrial restructuring and production reorganization. In so doing, this volume enriches a long line of literature (from the French regulationists to Piore and Sabel to Kochan, Katz and McKersie to Wolfgang Streeck), tracing the breakup of postwar Fordism and the uncertainties and employer offensives of the new post-Fordist era. The authors identify both the effects of this sea change on North American labor and contrasting union responses, linking restructuring to the union drive to build political alliances and organize this era's key new groups of workers, such as women in the service and public sectors (in chapters by Tanguay, Saint-Pierre, Richard Valelly, Elaine Bernard, Ruth Milkman, Julie White, and Charlotte Yates).

Finally, on a groundwork of solid evidence and analysis, in chapters by Jenson and Mahon, Milkman, Yates, Lipsig-Mumme, Donald Wells, Stephen Herzenberg, Richard Freeman, and Donald Swartz, the book articulates the outlines of a much-needed vision of the future for North American labor. To survive as more than a marginalized force, unions in the United States need to move beyond defensive cooperation, just as unions in Canada need to move beyond defensive opposition. Both need to go proactive: If management's Toyota-inspired "team concept" is not the answer, both to empower workers and at the same time contribute to industrial success, then what is? If the North American Free Trade Agreement is not the answer, what combination of micro- and macroeconomic and trade policies makes sense? While this book points to various possible answers, the central message is that these are not questions for top labor leaders to sit down and answer themselves. Rather, unions need to democratize and organize; bring in new groups of workers; open up union bureaucracies to active participation from below; and, in the process, articulate a politically viable vision that can claim broad support and for which many will mobilize. If unions fail to do this on a large scale, the curtains of history may be closing on organized labor, in *both* the United States and Canada.

Missing from this volume is a broader comparative context in which to understand North American labor. Rogers offers a framework, but it is not picked up on anywhere else. We do not know, for example, whether the theoretical claims made by Robinson, Bruce, Meltz, or Wells would stand the test of comparison beyond the two cases offered. An additional chapter could have been added to provide a broader comparative analysis—in particular, to countries of Western Europe and Japan. Or else, the editors could have done this in a concluding, synthesizing chapter à la Peter Katzenstein, in which the volume's findings were woven into one large and potentially provocative tapestry. Such a chapter could also have drawn out and addressed the debates that are present in this book but not sharpened, such as contrasting causes for American union decline offered by Robinson, Rogers, Valelly, Bruce, Meltz, Wells, Herzenberg, and Freeman.

The book is first-rate and highly recommended for anyone with an interest in contemporary comparative political economy, processes of industrial restructuring, or industrial relations and the fate of contemporary labor unions.

Cornell University LOWELL TURNER

For Whose Protection? Reproductive Hazards and Exclusionary Policies in the United States and Britain. By Sally J. Kenney. Ann Arbor: University of Michigan Press, 1993. 375p. $49.50 cloth, $18.95 paper.

In 1978, five women who worked in the lead pigments department of the American Cyanamid Company were sterilized in order to keep their jobs. They took this drastic measure in response to American Cyanamid's "exclusionary policy." This policy prohibited fertile women from working in areas that would expose them to substances suspected of being reproductive hazards. In her book *For Whose Protection?*, Sally Kenney examines this incident and other exclusionary policies in the United States and Britain that restricted women's access to the workplace.

By placing U.S. and British exclusionary policies in the historical context of protective legislation for women workers, Kenney shows how contemporary efforts to exclude women from the workplace are linked to protective measures of the past. Protective laws in both countries were based on the view that biological differences between women and men could justify differential treatment in the workplace. Yet in Britain, judges played no part in the battles over the legality of such policies. In the U.S., on the other hand, "many important battles over protective legislation took place in the courts" (p. 41). This was also true for the battles over contemporary exclusionary policies. It is these legal battles that comprise the central focus of the book. Kenney analyzes a small sample of cases from Britain and the U.S. to explore how judges, feminist groups, and agencies enforcing workplace health and safety responded to claims that exclusionary policies violated British and U.S. anti-discrimination laws.

Kenney shows that in rulings on exclusionary policies, U.S. judges have been less likely than members of British tribunals to uphold the differential treatment of women and men. Judges in the U.S. were more careful in scrutinizing employers' rationales for exclusionary policies and were more likely to grapple with complex scientific evidence regarding reproductive hazards in the workplace. Nonetheless, before the Supreme Court ruled in 1991 that exclusionary policies violated Title VII, some U.S. judges were willing to uphold policies that excluded women from the workplace.

Interestingly, neither feminist groups nor enforcement agencies in Britain made the issue of exclusionary policies "a priority in litigation or lobbying for legislation" (p. 322). In the U.S., on the other hand, feminist groups were at the forefront in challenging exclusionary policies in the courts. The U.S. Equal Employment Opportunity Commission also opposed exclusionary policies, though its role in the matter shifted under different presidential administrations.

The concept of discrimination and theories about difference and equality form the theoretical basis of this study. Drawing from scholarship in feminist theory, Kenney argues that exclusionary policies "reflect gendered thinking" and that the judicial response to such policies "exposes the inadequacy of the concept of equality upon which our ideas about discrimination rest" (p. 2). Exclusionary policies were defended by employers and upheld by some judges because they were viewed as appropriate responses to "real" differences between women and men. The debate over exclusionary policies raises the question whether it is lawful to discriminate against women who are different from men biologically, but who would suffer a disadvantage in the workplace because of differential treatment.

This "equality riddle" has been the subject of extensive debate by feminist legal scholars. Those who adopt the "special treatment" approach claim that sometimes women should be treated differently than men to attain equality with men. Advocates for the "equal treatment" approach argue that emphasizing the differences between women and men can reinforce stereotypes that become embedded in the law and be used against women to oppress them. In Britain, members of industrial tribunals were "not very suspicious of differential treatment on the basis of sex" and ruled that exclusionary policies were lawful (p. 324). Although the U.S. Supreme Court rejected exclusionary policies, its ruling was based more on conventional statutory interpretation than on an analysis of the equality debate and how the law should respond to claimed differences between the sexes.

The theoretical underpinning provides both the strengths and weaknesses of this book. Kenney provides a sophisticated analysis of the "dilemma of difference" and its implications for the development of sex discrimination law in the U.S. and Britain. However, this theoretical framework prevents Kenney from acknowledging that concern for fetal health and fetal rights might also have played a role in the debates over exclusionary policies. She uses the term "exclusionary policies" instead of the term "fetal protection policies" because she does "not see such policies as protecting the unborn but as discriminating against women" (p. 1). The evidence marshalled in the book clearly supports this view. Yet it is possible that "fetal protection" was a concern of employers and judges. Kenney fails to recognize this possibility when she quotes one judge who remarked that "This is the case about the women who want to hurt their fetuses" (p. 258). Moreover, some interest groups that participated in the litigation in the U.S. employed "fetal rights" discourse in advocating that it was not unlawful to exclude women from the workplace.

An additional weakness of the study is the lack of attention to the intersections of race and sex. While Kenney does address the class dimensions of protective and exclusionary policies in the U.S. and Britain, she does not extend this analysis to the racial dimensions of these so-called protective policies. Media accounts of some exclusionary cases in the U.S. reveal that many litigants were women of color. Yet historically, women of color were rarely "protected" by law from workplace hazards. Incorporating race into legal analyses of sex discrimination law can be a complicated undertaking. Feminist scholarship must, however, move in this direction.

These shortcomings do not diminish the important contribution this book makes to legal studies of women's rights. It is a fine example of the use of feminist scholarship to critique the development of sex discrimination law and policy. Moreover, readers will find that the book challenges us to think about how we might develop "a feminist re-vision of equality" (p. 9).

University of Georgia KAREN J. MASCHKE

Judicial Power and the Charter: Canada and the Paradox of Liberal Constitutionalism. By Christopher P. Manfredi. Norman: University of Oklahoma Press, 1993. 292p. $19.95 paper.

Canada's enactment in 1982 of the Charter of Rights and Freedoms has predictably had a profound effect on the evolution of constitutional jurisprudence in that country. It has also provided scholars with another opportunity to explore the theoretical and practical implications of the countermajoritarian logic of judicial review. This is the task set out in Christopher P. Manfredi's provocative and controversial analysis of the Charter and its importance for "the paradox of liberal constitutionalism," which, as presented here, refers to the possibility that "judicial enforcement of rights may destroy the most important right that citizens in liberal democracies possess, i.e., the right of self-government" (p. 37).

This is familiar terrain that Manfredi traverses competently, displaying solid grounding in the debates within American constitutional theory. He wastes no time in identifying himself with the critics of judicial activism and noninterpretive jurisprudence, aligning himself most closely with the argument for structural restraints on judicial power (as opposed to judicial self-restraint) that has been forcefully articulated in John Agresto's *The Supreme Court and Constitutional Democracy* (1984). This orientation is lucidly (if somewhat laboriously) revealed in Manfredi's detailed discussion of many Canadian Supreme Court cases. Most interesting for American students of constitutional law is his analysis of how certain distinctive features of the Canadian Charter have facilitated the judges in Canada in their steadily increasing drift toward a posture of judicial supremacy that mirrors what the author sees as some of the worst judicial excesses of their colleagues to the south.

For example, Section 1's provision that all of the guarantees of rights set out in the Charter are "subject only to such reasonable limits prescribed by law as can be demonstrably justified in a free and democratic society" has had the effect of emboldening, rather than constraining judicial power, leading the courts to adopt a public-law litigation model that is inappropriate for the tasks of traditional adjudication. Other sections, as well, (e.g., those ensuring that liberty will not be deprived "except in accordance with the principles of fundamental justice" and that laws seeking the amelioration of conditions for certain specified groups will not be invalidated) have contributed to the construction of a results-oriented jurisprudence that directly challenges the core precept of liberal constitutionalism.

There is much of great value in this study, some of it ironically the result of its principal weakness as a work of comparative constitutionalism. The author rightly devotes considerable attention to American approaches and cases (as have the Canadian courts), but he has very little to say about the contrasting political cultures that give definition to the respective constitutional experiences of the two polities. He has some thoughtful things to say about communitarian constitutional theorists who have challenged the Charter from the Left, but his quick dismissal of their position deflects him from addressing the legitimate issues of national identity and constitutional culture that are implicit in their critique. Section 15's provision for equality rights may very well have created, as Manfredi maintains, an alternative method for pursuing interests that have failed to prevail in the political process. However, such a development only raises the question of whether this is not, in fact, in accord with the liberal constitutionalism of the Charter itself. The author's claim that the judges have largely ignored the meaning of the Charter's drafters would be much more persuasive if there were more ample reason to think that these drafters had not envisioned their project as an attempt to create a distinctive experiment in liberal constitutionalism that might advance the cause of that most elusive of goals, the clarification (if not the creation) of Canadian national identity.

What is perceived, then, as the paradox of liberal constitutionalism requires a subtler analysis of the possible forms that such constitutionalism might assume. The "free and democratic society" that serves as the limiting principle of Section 1 may connote something significantly different from what it signifies in the American constitutional tradition. This is not to deny that there is some irreducible minimal content to what "liberal constitution" signifies, only that significant substantive alternatives may be accommodated within the generic term. The contrast is often made between the melting-pot aspiration of American ideology and the vision of Canadian society as a *mosaic* of different groups. In truth, this contrast is often more apparent than real; yet its constitutional prominence—reflected in Section 27's requirement that the document be interpreted "in a manner consistent with the preservation of the multicultural heritage of Canadians"—should make one pause before embracing a uniform and largely undifferentiated definition of liberal constitutionalism.

It is in this context that Manfredi's concluding reflections upon the Charter's most unique feature, the "notwithstanding" provision of Section 33, need to be considered. This section provides for a legislative override, whose legitimacy Manfredi defends with considerable eloquence. Indeed, his observations here with regard to the equal responsibility of the legislative and executive branches in determining the meaning of the Charter is an important contribution to comparative constitutional analysis, from which Americans, in particular, have much to learn. He rightly finds value in the authority that the section provides for the reassertion of the democratic will against the judicial will. It "can have a positive impact by encouraging a more politically vital discourse on the meaning of rights and their relationship to competing constitutional visions" (p. 207). Recognition of this potential should, however, alert us to the possibility that the judicial will *may*, on occasion, be the carrier of the democratic aspirations of the regime. Perhaps especially in Canada, this, in the end, might be the ultimate paradox of liberal constitutionalism.

Williams College GARY JEFFREY JACOBSOHN

Social Corporatism: A Superior Economic System? Edited by Jukka Pekkarinen, Matti Pohjola, and Bob Rowthorn. New York: Oxford University Press, 1992. 430p. $95.00.

The product of a collaborative research project by British and Finnish economists, this volume explores the economic implications of corporatist political arrangements in general, and centralized wage bargaining in particular. The volume considers a wide range of economic performance indicators, but focuses on employment and the distribution of income and consumption. The key question is, in the words of Andrew Glyn, "who bears the costs of slower growth?"

The length and price of this volume are daunting, to say the least, but reading it is well worth the effort, and I am very glad to own a copy. Though it appears overgrown and somewhat untidy at first sight, this is one of the most coherent edited volumes that I have ever read. It represents a major contribution to the literature on corporatism; in my view, the most important book on the subject since Peter Katzenstein's *Small States in World Markets* (1985).

The volume is first and foremost distinguished by its systematic use of cross-national comparative analysis. Focusing on Katzenstein's four cases of "social corporatism"—Sweden, Norway, Denmark and Austria—plus Finland, the volume not only compares these countries to each other, but also to other small European countries (Katzenstein's cases of "liberal corporatism") and to the large OECD countries. The upshot is a staggering amount of cross-national statistical data on growth, employment, industrial structure, wage differentials, etc. While the quantitative data compiled does not always speak to the arguments advanced, several of the essays present fresh data, carefully crafted to address the issues at hand. (Bob Rowthorn's Index of Employment and Earning Dispersion, a.k.a. the INDEED index, deserves special mention.)

Framed in terms of current debates among economists on the relationship between economic performance and wage bargaining institutions, the volume also speaks to the concerns of political scientists. Here Katzenstein's work serves as the principal reference point. In one of the country-specific essays towards the end of the volume. Wolfgang Blaas takes issue with Katzenstein's characterization of Switzerland as corporatist, and argues that Switzerland should rather be conceived as a case of "liberal capitalism." The Swiss case raises the question of the importance of the distinction between "social corporatism" and "liberal corporatism," relative to the distinction between corporatist and non-corporatist political economies, in Katzenstein's work, and more generally in the literature on corporatism.

More importantly, however, this volume brings out the challenge that the Austrian case poses for Katzenstein and other students of corporatism. As everyone agrees, Austria is the most corporatist of all the corporatist countries if corporatism is measured in terms of the union participation in economic and social policy-making and/or the degree to which wage bargaining is centralized. Yet Austria's employment performance diverges sharply from that of the Nordic countries. Among the OECD countries, Austria has one of the lowest rates of labor force participation, and one of the highest rates of wage dispersion, with especially high male-female differentials.

Comparing Austria to the Nordic countries, one must conclude that similar institutional arrangements can yield very different employment outcomes. At the very least, the analytical primacy typically assigned to institutional arrangements in the literature on corporatism must be qualified. According to the contributors to this volume, the divergence in employment performance between Austria and the Nordic countries hinges on the *policy objectives* pursued by political-economic actors within centralized bargaining structures. Rowthorn, in particular, distinguishes between "inclusive" and "exclusive" policy orientations. For Rowthorn, this distinction pertains not only to union wage demands (the absence of wage solidarity in Austria), but also to a wide array of government policies that affect the degree of labor-market segmentation, and especially the labor-market opportunities for women.

In a provocative essay, which stands in a rather uneasy relationship to the volume as a whole, Goran Therborn argues that the concept of corporatism conflates two fundamentally different phenomena: the "institutionalization of conflict" and the "institutionalization of consensus." While the Nordic countries fit the former category, Austria and most other continental European countries fit the latter. Building on Therborn's conceptualization, Rowthorn suggests that the institutionalization of conflict creates a dynamic that forces labor movements to pursue more solidaristic policies, presumably because the exercise of power vis-a-vis capital depends on the mobilization of broad-based support. Be that as it may, the bottom line here is that "Austrian exceptionalism" derives from the dominance of skilled, male workers within the labor movement.

Rowthorn and others suggest that the contrast between Austria and the Nordic cases bears not only on employment and income distribution, but also on economic growth and restructuring. The severity of the structural crisis of the Austrian economy in the 1980s, they argue, can in part be explained by the resistance to change by well-paid workers in older, nationalized industries. In Sweden, by contrast, wage solidarity and active labor market policies have facilitated restructuring.

For all its insights, this volume leaves us hanging, for it does not really address the question of the day: If "social corporatism," as practiced in the Nordic countries, is indeed a superior arrangement from an economic as well as a social point of view, why is it in retreat? Several of the contributors note the political difficulties of Social Democracy, and the policy changes that have occurred in the Nordic countries since the early 1980s, but their impressionistic and speculative treatment of the apparent crisis of social corporatism stands in sharp contrast to their rigorous analysis of the economic consequences of social corporatism in the period from the mid-1960s to the mid-1980s.

Cornell University JONAS PONTUSSON

Citizenship and Employment: Investigating Post-Industrial Options. By Jocelyn Pixley. New York: Cambridge University Press, 1993. 339p. $54.95.

Jocelyn Pixley's book deals with one of the crucial issues of the 1990s. By the end of 1993 unemployment is forecast to reach 36 million in the OECD countries, yet there has been relatively little popular or governmental concern over the past two decades about the reemergence of mass unemployment and its implications for democracy and citizenship. Pixley surveys and criticizes the arguments of the 'post-industrialists', including Gorz and Offe, who believe that structural economic changes have made present unemployment levels irreversible. They call for a redefinition of 'work', the separation of income from employment through the development of alternatives to wage labor, and the creation of new political arenas as a way of empowering the unemployed. Pixley, in response, argues that the post-industrial strategy undermines citizenship and deepens the exclusion of already marginal groups. Full employment is still a feasible policy and is the only means through which citizenship can be secured.

One of Pixley's main criticisms of the post-industrialists is that they neglect the role of the state. Their policies can be, and invariably are, turned in other directions by state intervention that is required to implement them. Pixley takes as 'counterfactual' case studies the Nixon proposal in 1969 for a guaranteed income in the form of the Family Assistance Plan, and the promotion of communes and worker cooperatives by state and federal governments in Australia in the late 1970s and 1980s, all of which were failures. She uses these examples to show that governments have different aims from the post-industrialists in supporting these policies. Governments aim, for example, to reduce inflation, to lower public spending, to privatize services, or reduce the number of female-headed households. The case studies, while interesting, hardly support the general conclusion that because governments with certain economic views see, say, the promotion of worker cooperatives as a way to reduce the deficit from cash transfers rather than as a way to increase democracy, this will always be the case. Policies can be advocated for a number of different reasons, and a large part of politics is concerned with attempts to ensure that particular reasons or particular interpretations prevail. Indeed, Pixley's insistence that full employment is possible, implies that there will be governments willing to implement the necessary policies.

Pixley makes some telling points against the post-industrialists, but there are two major problems with her discussion. First, little evidence is produced to convince the reader of her case. Apart from the 'counterfactual' examples just mentioned, the book is exclusively focused on theoretical issues (and burdened with a cumbersome reference system). Given her thesis and the length of the book, one could reasonably expect that a few of the most relevant factors could at least have been canvassed. For example, what counts as 'full employment'? Do part time jobs count? What is the character of the service sector? Is it composed merely of 'junk jobs'? What weight should be given to the international context of employment (which Pixley does not mention)? She points out that some countries, notably Sweden, maintained high employment levels in the 1970s and 1980s, but will Sweden in the 1990s give support to Pixley's argument? The 'Swedish model' began to come unglued in the mid-1980s, unemployment has now reached 7.5% and government expenditure is being curtailed.

The second problem with the book is the treatment of citizenship. As Pixley points out, there has been and still is a very strong connection between employment and citizenship—but the fundamental question is whether such a connection should dominate democracy into the 21st century. Pixley's central assumption is that there can be no genuine citizenship unless all adults are employed. What she does not consider is whether, *irrespective* of employment trends, there is one element, at least, of the post-industrial strategy that should be taken very seriously by anyone concerned with strengthening citizenship.

Pixley places considerable weight in her criticism of the post-industrialists on the claim that a guaranteed income will not assist the unemployed; "a guaranteed income *in general*, or in any context, offers no progressive solution for marginal populations" (p. 121). Only employment plus good unemployment benefits to allow a choice of jobs, she maintains, can underwrite citizenship. She also states that 'future implementation' of a guaranteed income is "most unlikely given the nature of

[Western] governments" (p. 91). But as I noted above, full employment also depends on a big change in government outlook, so that is hardly a decisive objection. One might also add here that those presently employed seem uninterested in including anyone else; for instance, the proposal by Nordic women for a six hour working day receives little support from the employed or their organizations.

One reason Pixley dismisses a guaranteed income is that she fails to make an important distinction. A 'guaranteed income' can refer either to an extension of the old British Poor Law, that is, to a safety net below which the poor do not fall; or it can refer to a basic income for everyone, to an entitlement of citizenship. Pixley argues that full employment must be accompanied by good unemployment benefits to allow choice of work, but 'benefits' are not an entitlement, and are available only to those in employment, not to all citizens. The idea of a basic income is far from new—in 1919, one advocate called it "the monetary equivalent of the right to land, or of the right to life and liberty"—and there is now considerable interest in the idea in Europe. A basic income as a right of citizenship (or of permanent residence), far from being inimical to democratic participation, would provide a necessary resource for citizenship.

As a strategy to weaken the link between income and employment, introduction of a basic income has another advantage. It would—if set at an appropriate level—make entry into paid work voluntary for the very first time, and would also help to undermine the subordination inherent in employment. Pixley mentions workplace democracy approvingly in passing, but she ignores the question of the structure of authority in the workplace and its impact on individuals' participation, skills and capacities, in short, the impact on democracy. A basic income would also give all women an independent income for the very first time, and thus help undermine another form of subordination. Pixley makes the puzzling claim that a basic income would make no difference to women because of their links to children. In general, her comments about women's citizenship are not well informed (for instance, the statement that women "gained the vote at the turn of the century" [p. 284], when enfranchisement ranged from 1893 in Aotearoa/New Zealand to 1971 in Switzerland) and she shows little appreciation of the problems for women's citizenship in closing off the possibility of new thinking about 'work' and moving away from employment as the central way of organizing work in society.

In large part in the Anglo-American countries, social (welfare) rights have not been entitlements of citizens but have been linked to employment—and to men's employment. Political theorists have paid remarkably little attention to the fact that many of the so-called rights or entitlements of citizenship are not anything of the kind, but linked to a 'contribution'. What the contribution is, who can make it, and why some contributions, notably those from women, are disregarded, are vital issues that are usually glossed over in discussions of citizenship. Pixley takes it for granted that the most important (or the only) contribution has to be employment—an assumption contested by feminists for at least two centuries. Moreover, entitlements of citizenship, if they are truly entitlements, cannot be linked to 'contributions' but must be provided as a right (compare the vote). A basic income would cost a lot, as Pixley duly notes, but so would full employment. Like other policies, a basic income is also advocated for a large number of different reasons. The democratic reason is that it would enhance freedom and citizenship. An exclusive focus on employment is of limited assistance if citizenship is ever to be of equal worth to all members of the population.

University of California, Los Angeles CAROLE PATEMAN

Regional Development in Communist Yugoslavia: Success, Failure, and Consequences. By Dijana Plestina. Boulder: Westview Press, 1992. 223p. $38.00.

Dijana Plestina has composed a cogent study which scrutinizes the origins and implications of regional economic disparity in the former Yugoslavia. She interviewed in depth and over a number of years an impressive group of 105 Yugoslav luminaries involved at various times and in various stages of Yugoslav regional development from study to execution. Plestina confronts these experts with the problem, why did socialist Yugoslavia fail at every step in the post-World War II era to eliminate economic disparities among the various regions? While equalization of conditions for every region in Yugoslavia was mandated directly in every five-year plan, Yugoslav leaders were unable or unwilling to take the painful steps which would have narrowed the gap between the wealthiest regions and the poorest. Plestina argues that "economic regionalism, that is, the pursuit of one's own region's economic interests, explains better than any other factor the Yugoslav socialist regime's overall failure in narrowing regional economic inequalities" (p. 173, italics in original).

In pursuing this theme, the author examines her interlocutors' alternative explanations for the failure to close the gap between the wealthy and the poor regions. For example, a number of interviewees suggested that it was not economic regionalism but the Yugoslav fear of the Soviet Union after 1948 which forced Yugoslavia to concentrate on strengthening itself economically and politically to the detriment of the poorer regions. In this scenario, the less-developed regions, which could not in their poverty and under-capitalization measurably aid in the goal of forging a stronger Yugoslavia, would have to sublimate their aspirations until the external threat to Yugoslavia disappeared. But, says Plestina, this explanation is not convincing, for the threat to Yugoslavia abated measurably starting in the mid-1950s as the United States exerted itself economically and politically to protect Yugoslavia. Furthermore, Yugoslavia's activist foreign policy based on the nonalignment movement founded by Tito, Nehru, and Nasser provided it much political cover from the Soviet threat. And, in fact, when Khrushchev and Tito met in 1955 and 1956, the Soviet threat all but disappeared. But the regional economic disparities in Yugoslavia did not disappear. Here other explanations (read excuses) were tendered for this deficiency. These excuses, states Plestina, amounted at bottom only to the attempt by the wealthier regions to achieve the best economic position for themselves by capturing as many resources as possible for their own economic development without undue concern for closing the widening development gap with the poorest regions.

Plestina acknowledges the importance of ethnonationalism in the decision-making process within the former Yugoslavia but insists that "politicized ethnicity" appeared in response to threats to the economic well-being of a region. It is here that people might reasonably differ. For example, the author points out repeatedly that the exception to the rule of inequitable distribution of resources to the less developed republics was Montenegro, which consistently seems to have received an adequate if not generous amount of resources from the granting institutions. However, Montenegro and Serbia seem to consider themselves ethnically cousins if not brothers; furthermore, they were close comrades during World War II. Until the reforms of the mid-1960s in the former Yugoslavia, economic resources were closely controlled by the federal government centered in Belgrade. Since the Serbs and Montenegrins were generously represented in the political-bureaucratic apparat in Belgrade, one suspects that ethnonationalism was the primary motivation for Montenegro's more favorable treatment over the other poorer regions. Plestina points out that Croatia, which during its pro-Axis stage during World War II was an implacable foe of Serbia, was often ignored, or at least short-changed when economic resources were passed out. Kosovo also was penalized by those in charge of handing out the economic largesse because of its attempted wartime rebellion against Tito's partisans. These points, too, smack of an ethnic basis for regional development funds rather than the opposite.

Even after the institution of the political and economic reforms and the consequent devolution of political power to the republics, the resulting immobilism in federal affairs, exacerbated by ethnic-based politics, effectively froze the possibility of altering inter-republic and inter-ethnic relations. The least-developed areas continued to grow economically, but not nearly as quickly as the wealthier regions. The poorer areas watched helplessly as the development gap widened inexorably.

Finally, hindsight appears to bear out the primacy of ethnic factors over economic factors in decision-making in Yugoslavia as we now observe the destruction of the Serbian economy in order to attain questionable battlefield victories in Croatia, Bosnia and Herzegovina, and potentially in Macedonia and Kosovo. Were regional economic criteria primary, would Milosevic's blandishments have been so successful in enticing the Serbs to wreck their country to help pursue a war which would probably lead to their becoming a pariah to the international community and failing to obtain international aid to build up the "new Yugoslavia"?

While people might plausibly debate the primacy of regional economic concerns for policy decision-making versus ethnonationalistic criteria, the real value of Plestina's work is the insight she provides into the thinking over time of many of the decision-makers in the former Yugoslavia. As a Croatian citizen of the former Yugoslavia, she could be considered a concerned co-national attempting to ferret out reasons why the stated policies did not bring about the desired outcomes. Her respondents may have been more forthcoming to a compatriot and thus may have provided her with better and more honest insights into the problems of the former Communist regime than they might have been to a non-Yugoslav researcher.

The author painstakingly builds her argument about the persistent inequality among regions despite the regime's claims of a 45-year attempt at equalization by a careful definition of parameters and criteria for measurement. She also attempts to guard against inserting her own biases and makes extraordinary efforts to avoid the sort of theoretical fuzziness into which studies such as this so easily lapse. Plestina's efforts are commendable and this book should be read for its insights into the failure of the Yugoslav socialist experiment.

Ball State University FRANCINE FRIEDMAN

INTERNATIONAL RELATIONS

A Foreign Policy in Transition: Moscow's Retreat from Central America and The Caribbean, 1985-1992. By Jan S. Adams. Durham: Duke University Press, 1993. 248p. $39.95 cloth, $14.95 paper.

What a pleasure it is to read a thoroughly researched and well-argued book by a single author that puts Gorbachev's reconstruction of Soviet foreign policy into a timely perspective. Dr. Adams' study does far more than outline the evolution of Soviet policy toward Central America from Stalin's hostility, through Brezhnev's aggressiveness, to Gorbachev's new political thinking. All along, the author keeps in mind several pertinent questions that retain their relevance, both practical and theoretical, beyond the narrower focus of Moscow's retreat from the Caribbean area. Three basic questions underlie Adams' inquiry: What signs indicate that Gorbachev's reforms can survive beyond his tenure of power? What was there in the American assessment of Moscow's intentions that could have led to an exacerbation of superpower competition? And will the collapse of Communist system have any influence on the social, economic, and political tensions that afflict Central America?

The book offers a logical and interconnected presentation of Moscow's advances and withdrawal. The introductory chapter deals with the Stalin, Khrushchev, and Brezhnev legacies. The six chapters that follow neatly outline the basic reformulations and departures introduced by Gorbachev, his and Shevardnadze's reform of the Foreign Ministry; the impact of the changed course on relations with Cuba; the pivotal decision to accept a peaceful political solution in Nicaragua; and the ripple effect of new policies on Moscow's relations with other countries on the Isthmus as well as with other Caribbean islands. A conclusion discusses the prospects after the failed coup of August 1991.

Adams' overall assessment of Moscow's retreat is upbeat. Not only does she imply that the changes introduced by Gorbachev—his recognition of an interdependent world and his acceptance of a cooperative relationship with the US—are irreversible. She also holds that Andrei Kozyrev, the Russian Foreign Minister, wields unchallenged authority and that Moscow's policies will follow his expressed preference for a "civilized" international behavior that is based on the same system of values that unites the West and will make his country "a natural ally" of France, Germany, and the US.

This line of argument strikes one as too much like the "end of history" theory that became fashionable with the collapse of the Soviet Union as a country and of communism as a system. In stressing the role of ideology in the conduct of foreign policy, it tends to overlook the reality of national interests which elude ideological definitions. Dr. Adams' conclusions are especially surprising, given that she alludes to the variety of responses that arose in reaction to Gorbachev's foreign policy revolution, ranging, as they did, from the flagrantly chauvinist and imperialist to the truly international-minded. The original, pro-Atlantic, Kozyrev line has been modified considerably by late 1992, when conservative patriots, opposed to excessive cooperation with and reliance on the West; gained strength in the government and in Yeltsin's entourage. The signing, in December 1992, of the Moscow-Havana agreement on the continued Russian operation of intelligence facilities on Cuban soil is but one reminder that national and strategic interests will overridingly persist.

Despite this single example of a certain short-sightedness, Dr. Adams book, being the product of conscientious research and consistent analysis, is a valuable contribution to the study of Soviet and post-Soviet relations with Central America. It offers numerous insights that could guide Washington as it faces now a review and reformulation of policies toward Castro's Cuba.

The Harriman Institute ELIZABETH KRIDL VALKENIER
Columbia University

Human Rights in Cross-Cultural Perspective: A Quest for Consensus. Edited by Abdullahi Ahmed An-Na'im. Philadelphia: University of Pennsylvania Press, 1992. 479p. $32.95.

Human Rights and Statistics: Getting the Record Straight. Edited by Thomas B. Jabine and Richard P. Claude. Philadelphia: University of Pennsylvania Press, 1992. 458p. $36.95.

These two volumes in the new Pennsylvania Studies in Human Rights Series could hardly be more different; but taken together, they nicely summarize the current state of theory and method in the study and advocacy of human rights.

This is a field in its infancy; indeed, it may be premature to call it a "field" in social science at all. Not only is it new (the Universal Declaration of Human Rights, upon which most subsequent development has relied, was adopted only in 1948, and as serious American preoccupations, human rights are no older than the Carter administration) but its practitioners are self-consciously resistant to definitions of social-scientific research that characterize it as "value-free." As An-Na'im's volume so eloquently testifies, not only do normative debates abound, but biases are often proudly declared. Yet as the contributions to *Human Rights and Statistics* persuasively contend, there can be a "science of human rights."

The introductory and concluding essays to *Human Rights in Cross-Cultural Perspective* are by An-Na'im, a Sudanese law professor recently named executive director of Africa Watch; and they are eloquent (if inconclusive) explorations of the charge that human rights advocacy is but thinly disguised cultural imperialism. Because the philosophical tradition that gave rise to concern for human rights is most emphatically a Western one—embedded in, and perhaps necessitating, liberal individualism—they are often portrayed as foreign impositions in non-Western contexts. Moreover, the precepts of human rights sometimes directly contradict the imperatives of revealed religion. Is the Islamic injunction to cut off the hand of a thief "cruel, inhuman, or degrading punishment" by human rights standards? If it is, whose law should prevail? An-Na'im is particularly interesting to hear on this question because he is a practicing Muslim, a follower of Mahmud Muhammad Taha, a Sudanese Muslim philosopher executed for heresy in 1985. Although, like Taha, An-Na'im is prepared to go quite far in reinterpreting Islamic precepts in light of modern circumstances, unlike many secular advocates of human rights, he is not willing simply to abandon the religious and cultural imperatives of Islam altogether.

Several other essays offer similarly provocative discussions of the strengths and limitations of the liberal individualist bias in contemporary human rights law. Rhoda Howard and Michael MacDonald provide especially clear expositions of the communitarian critique of liberalism as it pertains to human rights, for example; and (perhaps stretching the notion of "cultural" somewhat to including ideological variations) Richard Nordahl and Tore Lindbolm explore Marxist approaches to the subject.

Unfortunately, the case studies, several of which are fascinating in their own right, do not illustrate the dilemmas An-Na'im outlines quite as well as they might. Of the seven empirically grounded essays, five-and-a-half are devoted to issues probably better described as cultural survival: the rights of aboriginal communities in North and South America and Australia to autonomy and self-determination. Although these are important questions, the dilemmas that face the indigenous populations in these centuries-old settler colonies are not, in fact, typical of the cross-cultural debates about the compatibility of, say, Islamic or Confucian values and human rights. Since claims to cultural authenticity are often little more than flimsy excuses for unprincipled disregard for elementary human rights, these debates are probably best joined when the integrity of the culture in question is greatest. Yet, the independence and coherence of native North American value systems have been profoundly eroded by four centuries of domination and exploitation. As a result, James W. Zion's proposition that there remain "base values" shared by all of the very disparate North American "nations, bands, tribes and groups," for example, and that these are inconsistent with Western liberalism (p. 192) is simply not compelling. These "base values" sound very much like ordinary survival strategies of underclass communities around the world—moral economies, if you will; and however important they are, they should not be confused with autonomous cultural traditions.

The absence of appropriate case studies weakens the argument for culturally determined resistance to human rights by suggesting, however inadvertently, that culture is little more than the last resort of rogues. In fact, as An-Na'im's own discussions illustrate, questions about the compatibility of human rights precepts with local belief and practice are not merely convenient ideological smokescreens but very important philosophical, even theological, quandaries.

Moreover, there are some rights about which there is relatively little philosophical debate: none of the governments that routinely resort to torture claims that it is culturally sanctioned. It is this universal agreement on at least some "rights" that provides the conceptual and political opening for human rights advocacy even amid philosophical disagreement. Responsible and effective advocacy requires, in turn, responsible and effective research, or, (as the human rights lingo has it) "monitoring." It is to this subject that *Human Rights and Statistics* is devoted, but the book is more than that. It is a marvelous treatment of the difficulties and rewards of social science research in authoritarian polities, particularly in the Third World; and anyone whose teaching included social science research methods and methodology should be familiar with it.

If ever social scientists were faced with "messy" or "dirty" data sets, it is those trying to document the fate of the disappeared in Argentina, the victims of torture in Iran, the mass executions in Uganda, and so-forth. Governments not only do not publish these data but deliberately destroy, distort, and conceal the evidence. As a handbook on how to develop and analyze such material, this volume is invaluable to far more than the human rights community; for its "rules of evidence" apply equally well to all kinds of poorly documented economic, social, and political data. "The Limitations of Quantitative Data in Studying Human Rights Abuses" by Robert Justin Goldstein and "Use of Incomplete or Distorted Data in Inference About Human Rights Violations" by Douglas Samuelson and Herbert F. Spirer are models of intelligence and ingenuity. They should be read by every graduate student contemplating social science research outside the industrial world (and perhaps within it), whether on human rights or not.

There are also good essays treating the development of data, both "events-based" (patterns of violations) and "standards-based" (particularly, comparative or cross-national patterns of policy) and the analysis of that data. Clyde Collins Snow and Maria Julia Bihurriet's essay on the disappeared in Argentina, "Epidemiology of Homicide," is a particularly elegant example of empirical social science research and analysis. Thus, even for social scientists who do not find the policy issues implied in human rights particularly compelling, this book is an especially useful treatment of vexing research challenges.

For those of us who do find questions of human rights substantively, as well as methodologically challenging, it is very gratifying to note the high quality of the research being done. The human rights advocates and monitors whose work is displayed in these pages are also very professional social scientists.

Columbia University LISA ANDERSON

The International Law of Occupation. By Eyal Benvenisti. Princeton: Princeton University Press, 1993. 241p. $29.95.

The subject of this well-researched and carefully-written book will attract a great deal of interest among those particularly interested in questions of international law or problems of military occupation; it should also attract wider attention because it has much to offer general students of international relations, especially those interested in the evolution and effectiveness of norms of acceptable state conduct. It is a valuable account of the traditional international legal norms regarding the protection of civilians in an occupied area and the changes that have occurred in these rules in the twentieth century as well as the possible emergence of new rules having their origin in the field of human rights rather than the law of war.

The older literature on military occupation consists generally of legal analyses and reflects the traditions and assumptions of the Hague Regulations. More recent literature tends to be either historical accounts focussing on specific examples of the occupation of a particular state or legal analysis of the Israeli occupation of the West Bank, Gaza, and Golan Heights. As a systematic and comprehensive review of the law and historical cases of military occupation since World War I, Benvenisti's study is a valuable contribution to the literature because he offers an overview of the problems, examines the relevance, application, and effectiveness of the international law rules, and traces the evolution of the applicable legal norms.

The rules of international law deal with military occupation and are traditionally considered to be part of the law of war. The first of these rules, the Hague Convention Regulations, sets forth the ideal of the military occupant as the trustee of the occupied territory exercising minimal interference in the lives of the local population but also possessing sufficient authority to protect its military operations. The matter of military occupation is a matter between the adversary states to be resolved after the conclusion of the conflict. Such occupation is not considered to lead to permanent changes in territorial control unless the changes are agreed to in a peace agreement. The newer rules set forth in the Fourth Geneva Convention attempt to provide a bill of rights for the local inhabitants and thereby shift the emphasis away from military occupation as a matter between states to a concern with the rights of the local population. Unfortunately the newer rules do not provide an effective formula to balance the competing political and economic interests of the local population and the military and political interests of the occupation forces.

Although these rules receive dutiful lip service from states, Benvenisti illustrates that in most instances the occupant state either denies the applicability of the rules or ignores them to pursue its own agenda in the occupied territory. The occupant is no longer an objective, if not disinterested, trustee; rather the occupant has its own political, economic, and security agenda which may not be compatible with the interests or needs of the local population. The application and effectiveness of the traditional rules have also been complicated by the willingness of either the occupant or the local population (in some cases both parties, e.g., Bangladesh) to reject the law of war and to assert alternative claims under international legal norms promoting human rights, including the right of self-determination of the local population against the original state. This trend, as well as the trend toward prolonged military occupation, cannot be ignored and brings into focus the need for the international community to reexamine the relevance or effectiveness of the traditional rules.

The author reviews a number of examples of military occupation ranging from the German occupation of Belgium in World War I to recent occupations in Afghanistan and Grenada, and devotes the greatest attention to the Israeli occupation of the West Bank and Gaza. Minimal attention is given to the issues of East Jerusalem and the Golan Heights. The decisions and policies of the Israeli government with regard to these territories have certainly been controversial and subject to extensive scholarly examination. Perhaps more than any other case, the post-1967 Israeli occupation illustrates the problems of reconciling the rights and long-term interests of the inhabitants with the security and other interests of the occupant and calls into question the effectiveness of the traditional rules in limiting or controlling the actions of the occupant. In general the Israeli policy appears to be to minimize the relevance of rules of international law and to maximize its authority as occupant. The government argues that the Hague Regulations are applicable to the occupied territories but the Geneva IV rules are not. Although the willingness of the government to recognize the applicability of the Hague Regulations is unique among all of the post World War II occupations, the assertion regarding the Geneva IV rules is not shared by the other members of the international community; both the Security Council and General Assembly have consistently called upon the Israeli government to accept the de jure application of the Geneva Rules to the territories.

The author does not appear to share the view of the government on the applicability of the Geneva IV rules and challenges the logic of accepting the Hague Regulations and rejecting the newer Geneva IV rules. With regard to the question of the West Bank settlements, the author concludes that the rules of occupation do not sanction such settlements. He also concludes that the extension of Israeli economic policies and taxes into the territories amounts to "economic annexation."

Unlike other instances of occupation the Israeli Supreme Court will consider appeals from inhabitants of the occupied areas regarding alleged illegal or arbitrary acts by the military authorities. The courts will consider claims under the Hague Regulations because they are considered to be part of customary international law, but will not adjudicate claims under Geneva IV rules because they are conventional rules between states and are not part of Israeli internal law. In general the effectiveness of this right of judicial appeal has been limited with the courts relying primarily on local law and security enactments.

The doctoral dissertation on which this book is based was submitted to Yale Law School and won the Ambrose Gherini Prize at that School. It contains some of the useful characteristics of a dissertation: an extensive bibliography and a valuable listing of relevant cases.

University of Maryland Don C. Piper

Export Controls in Transition: Perspectives, Problems, and Prospects. Edited by Gary K. Bertsch and Steven Elliott-Gower. Durham: Duke University Press, 1992. 355p. $57.50 cloth, $21.95 paper.

What are the implications of a post-Cold War world for national and multilateral export control policies? This volume, one of several produced in recent years under the auspices of the Center for East-West Trade Policy at the University of Georgia, brings together scholars and practitioners to address that question. Their underlying premise is that current export control arrangements reflect, in the words of the editors, a "system under stress" (p. 3) that needs to be reformed and adapted in order to remain viable in the new strategic and economic environment of the 1990s. The individual contributions, generally of high quality, explore the perspectives of different states (with heavy emphasis on the United States and its CoCom [Coordinating Committee For

Export Controls] partners), the export control problems they currently face, and their prospects for solving them.

Two important themes recur across the chapters. First, the contributors stress the need to move beyond the common conception of export control policy as involving a simple trade-off between economic benefits and security risks. William Long argues effectively that proposed technology transfers could just as easily involve trade-offs between security benefits and security risks. U.S. officials must now consider whether to transfer state-of-the-art supercomputers to the former Soviet Union in order to improve the safety of civilian nuclear reactors, or militarily critical sensor technologies to bolster confidence building measures in Europe. Similarly, Beverly Crawford points to the problems raised by "triple-use" technologies such as telecommunications which have potential risks and benefits across commericial, military, and political arenas, while Christine Westbrook and Alan Sherr explore the challenges posed by the recent proliferation of joint ventures. The upshot is that export control policies now require more complex calculations than they did during the Cold War, and the problem is complicated further because the trade-offs appear differently from the vantage points of Washington, Bonn, Tokyo and Seoul. The inclusion of the latter suggests, as argued by Han Park, that newly-industrializing countries have emerged in the 1990s as autonomous actors in the multilateral politics of export control.

Second, the authors show that control policies and regimes must respond simultaneously to structural changes in both economics and security systems. The end of the cold war and diminution of the Soviet military threat have combined with the decline of U.S. economic hegemony, the rise of interdependence, and the diffusion of critical technologies. The implications are profound: there is no longer a consensus on the purpose and targets of export controls; the United States may be unable to persuade or coerce other states; and the need for multilateral coordination is now greater as multilateral export control institutions have become weaker. National policies and multilateral regimes must adapt. Yet, as Kevin Lasher and Kevin Quigley argue, the U.S. control system has been incapable of reform in the past, and institutional factors such as the diffusion of decision-making authority and the vagueness of statutory guidance may impede adjustment in the future. CoCom may have difficulties as well, if it attempts to shift to a North-South from its traditional East-West focus.

Several shortcomings mar this otherwise useful collection of essays. As is typical of edited volumes, the individual chapters do not engage each other effectively. Authors take different positions, for example, on the crucial question of how much export controls mattered in the cold war military equation, but that debate is never joined. There is needless repetition as several chapters review in similar fashion the postwar history of export controls. And, though obviously no fault of the authors, the fact that the manuscript went to press prior to the collapse of the Soviet Union renders somewhat awkward the one or two essays which counsel Western vigilance in the face of a continued Soviet military build-up and threat under Gorbachev.

A more important limitation is that the volume as a whole lacks an explicit conceptual link to broader literatures in international relations and foreign policy. Several of the individual essays accomplish that: for example, Beverly Crawford uses complex interdependence to explain export control policy change, Henry Nau links export control regimes to particular conceptions of deterrence, and Richard Cupitt points to different assessments of CoCom's future under realist and institutionalist assumptions. The collective enterprise, however, comes up short in this regard and hence will be more valuable to the specialist than to the international relations generalist.

Dartmouth College MICHAEL MASTANDUNO

International Cooperation and Public Goods: Opportunities for the Western Alliance. By Mark A. Boyer. Baltimore: Johns Hopkins University Press, 1993. 176p. $33.00.

The application of public goods models to alliances represents more than a quarter-century's work. Starting from the original formulation by Mancur Olson and Richard Zeckhauser, a variety of tests, critiques, modifications, and extensions have been offered. At its best, the body of work provides an example of the scientific endeavor—cumulative research done on the basis of some generally agreed-upon agenda and issues. Boyer's book continues in this excellent tradition by broadening the scope of the discussion to include public goods beyond those directly relating to the security of NATO.

Boyer contends, correctly, that research on public goods and alliances has centered far too much on security matters. Certainly, the provision of security is the primary function of NATO; but the network among the member states that the alliance has fostered allows us both to see how other public goods are provided and to determine whether free-riding characterizes as much of international relations as the original Olson–Zeckhauser model implies. This is an important issue that goes to the heart of much of our understanding of how cooperation among states occurs. Thus, Boyer's work is significant for what it tells us about the less obvious forms of cooperation that may permeate international relations.

The heart of Boyer's argument is that many public goods, not just security, are provided by the Western alliance. Much of the research on the subject assumes that security is the only public good that matters; that since the United States provides a disproportionate share of that security, the other members of the alliance are free-riding; and that the provision of the public good is thus suboptimal. Boyer challenges these assumptions by using free-trade theory's concept of comparative advantage to develop his notion of *political comparative advantage*, which concludes that "policy constraints produce conditions that can facilitate a division of labor among the allies regarding alliance security" (p. 45). Essentially, these constraints mean that some states will find it relatively more costly to produce some goods than others. Boyer demonstrates the existence of these policy constraints through analyses of public opinion data within alliance members—analyses that conclude that there are a "diversity of security preferences among nations and . . . specialization tendencies within the alliance" (p. 63). For instance, Dutch public opinion approves of foreign aid to a much greater extent than do Americans; and in accordance with Boyer's argument, in 1987, the Netherlands spent almost 1% of its gross domestic product on Official Development Assistance, compared to .2% for the United States. Similarly, relative spending on research and development demonstrates some patterns that can be explained by Boyer's political comparative advantage. Boyer also includes an enlightening chapter on international monetary cooperation to demonstrate the multidimensional character of burden sharing.

Boyer's book has three flaws, two of them relatively minor. In his literature review section, Boyer relies too uncritically upon the work of others. For instance, Boyer's model maintains that security is provided through a variety of policies and that the analyst must put these policies together to achieve a full understanding of the public goods mechanisms underlying the alliance. To do this, Boyer unnecessarily and unfortunately ties his model to past efforts (particularly the joint-product model of Todd Sandler and his colleagues) to disentangle "security" into its components. It is unnecessary because Boyer's model has a definition of security that includes far more than its military component, while Sandler and associates are solely interested in the distinction between deterrent and defensive weaponry. It is unfortunate because Boyer paints a picture of cooperating states doing what they do best, while the joint-product model assumes the more traditional anarchic world of international relations. By tying his work so closely to this school, Boyer's contribution may be missed. The second minor flaw is the placement of his formal models in the appendices. These, to me, were the most interesting parts of the book; and readers would do well to consult them when reading the relevant chapters, rather than turn to them as an afterthought.

The more fundamental problem regards the characteristics of the goods being produced by the alliance. While Boyer admits that foreign aid and research and development are not pure public goods, they produce positive externalities in much the same way security does. But while security is provided knowingly and explicitly in order to be consumed by allies, foreign aid and research and development are much more private in nature; and their effects on allies is, at best, only a subsidiary consideration. An implicit and discernable division of labor on security matters is implied by its nature as a public good. But analyses of foreign aid and research and development are not based upon the clear testable propositions derived from the public goods paradigm. The conclusions of these analyses are, necessarily, more ad hoc and less than convincing. It is difficult to understand, for instance, why the Netherlands spends a relatively large proportion of its gross domestic product on foreign aid and research and development, while Belgium is only in the middle range. Boyer argues that preferences determine political comparative advantage, which, in turn, determines these relative expenditures. But this is the same as saying that they spend what they spend because they want to. Many of us base our expectations on assumptions regarding the preferences of the actors, but the tests of hypotheses derived from models built from those assumptions need to be rigorous and differentiating.

Nonetheless, Boyer's book is a fascinating and valuable contribution to the on-going work on alliances. The book should be carefully read by anyone interested in alliances, public goods, or formal models of international relations. His models provide what will become fruitful grounds for research and exploration; and for that, his contribution to the work in the field will be long-lasting and significant.

State University of New York, Binghamton GLENN PALMER

Guarding the Guardians: Civilian Control of Nuclear Weapons in the United States. By Peter Douglas Feaver. Ithaca: Cornell University Press, 1992. 261p. $34.50.

This book—part of Cornell's noted series in security affairs—concentrates on nuclear weapons operational policy, rather than the more familiar themes of strategy or mix of forces. Who has physical control of these devices? What balance has been struck in the world's foremost military power between conflicting claims? What are the implications of technological and strategic developments on the ability of decision-makers to utilize (or threaten the utilization of) nuclear weapons?

Feaver significantly enhances our understanding of "civilian control" in *Guarding the Guardians*. In *The Soldier and the State* (1957), Samuel Huntington asserted that "autonomous military professionalism" based on clear spheres of responsibility maximized national security. Huntington's distinction between "objective" (in Feaver's rephrasing, "delegative") and "subjective" civilian control has bedeviled the field of civil–military relations since. Herein lies the first major contribution of *Guarding the Guardians*. Feaver focuses on "assertive" means of control, which lie between wholesale delegation of responsibilities to the armed forces ("objective") and widespread, direct governmental involvement in decision-making ("subjective"). This intermediate way—defined as "direct civilian supervision over the military, particularly over military operations" (p. 9)—is marked by (1) qualitatively separate civil and military institutions, (2) overlapping roles and functions for them, and (3) presumptive conflict in civil–military relations.

Feaver enhances our conceptual awareness by presenting two dilemmas of control. First, nuclear weapons must be detonated only when directly ordered by the competent government authority. The "always–never" rule applies, meaning that (1) nuclear weapons *always* explode in the authorized fashion when authorized leaders so direct and (2) they are *never* detonated without proper sanction. Steps must be taken to preclude accidental use (safety measures engineered into weapons design), unauthorized use (an order emanating from below the level of the president or the secretary of defense), and third-party use (the bugaboo of covert penetrators, terrorist action, or host-nation takeover). Second, organizational and technological factors complicate the controller's tasks. Some "accidents" are normal, unavoidable, and unforeseen. Technological decisions carry policy implications—what Feaver deems the "politics of artifacts." Where military autonomy is reduced in one area, it may balloon in others. Greater efforts at control lead to endless cycles of interference and resistance. And as all persons knowledgeable of complex organizations realize, unwritten rules and "work-arounds" constrain whatever means of control exist.

Having established this intriguing series of principles and problems, Feaver examines operational factors.

Four issues are involved in any decision to use nuclear weapons: Who physically controls nuclear weapons? What is the line of presidential succession? How might command be devolved between levels in case of emergency? What authority should be delegated in advance? In this section, Feaver introduces the chief device used for assertive control, the permissive action link (PAL). Herein lies the (inaccurate) image of the button on the president's desk that, when pushed, leads directly and immediately to unleashing nuclear-tipped missiles. The reality is far more complex. Both authenticating and enabling codes exist. While the president can legally order use of nuclear weapons, actual detonation requires cooperation from officers who could conceivably ignore orders from above. Somewhere in the chain of command are persons who, while themselves unable to order the use of nuclear weapons, nonetheless hold "a significant amount of control over the entire sequence" (p. 39). Further issues arise when nuclear weapons are used: How and where are they targeted? What options can be exercised in the single integrated operational plan?

Concluding his theoretical section, Feaver proposes a model based on four independent variables: characteristics of the nuclear arsenal (notably its size and dispersal), its vulnerability, nuclear "doctrine," and presidential style. Shifts between delegative and assertive civilian control reflect the interactions of these four variables. The seven propositions he derives reflect good common sense and nuanced understanding of the inherent dilemmas of keeping the nuclear genie in its bottle.

I have focused on the broad questions Feaver raises and the policymaking model he proposes. This is not intended to slight other parts of his book, but to show the nuances of Feaver's analysis. His historical review follows in the same thoughtful manner (although formerly classified material was available to him only until 1962). The six chapters of part 2, The Evolution of Custody Policy, occupy nearly two-thirds of *Guarding the Guardians*. This section not only provides interesting details about policy evolution but also allows Feaver to test his four independent variables. As might be expected, the assertive control exercised when the American nuclear arsenal was small and Soviet threats limited, tended toward delegative control in the Eisenhower massive retaliation era, with a slight return toward the latter under Kennedy. Dramatic growth in the size, complexity and dispersal of the nuclear arsenal inevitably entailed growing use of PALs. The seven propositions that Feaver posits, overall, provide persuasive explanations of these developments (as handily summarized on pp. 220-22).

Guarding the Guardians thus merits attention on several grounds. Its detailed historical analysis provides some fresh details on previously plowed ground. Its formulation of civilian control introduces the useful category of "assertive" control. Its concluding suggestions for American policymakers ought to be pondered in Washington and other nuclear capitals. In short, *Guarding the Guardians* belongs on the shelves of major university libraries and interested scholars.

State University of New York, Buffalo CLAUDE E. WELCH, JR.

International Conflict Resolution: The U.S.–USSR and Middle East Cases. By Louis Kriesberg. New Haven: Yale University Press, 1992. 275p. $35.00.

Louis Kriesberg takes politics seriously. Peace does not come from some abstract "game" played by "actors" who adopt certain tactics suggested by group experiments. It comes, rather, through the slow accumulation of agreements, treaties, and settlements negotiated over specific issues. Building on I. William Zartman's (1985) *Ripe for Resolution: Conflict and Intervention in Africa*, and Janice Stein's (1989) *Getting to the Table*, Kriesberg raises a number of critical questions: what are the pre-conditions for "de-escalation" initiatives? Under what circumstances are such bids likely to lead to negotiations? When do negotiations lead to agreements? And, what is the cumulative impact of agreements on the overall conflict between two rivals?

Kriesberg's materials come from the U.S.-Soviet relationship between 1948 and 1989, and the Arab-Israeli conflict that endures to this day. The author examines three sets of background conditions that help explain successes and failures of de-escalation bids and negotiations: domestic constraints, the international context, and the nature of adversary relations. He also evaluates the predictive and explanatory capacities of four literatures that bear on the negotiation process: statism (realism), populism (idealism), pluralism (the impact of non-state actors), and conflict resolution (game theory, interaction analysis, and the like). He finds that most approaches make a contribution, but none is complete. Kriesberg makes a major contribution in showing the strengths and weaknesses of each.

What patterns does he uncover? There are, in fact, few. Kriesberg identifies 128 Soviet and American "de-escalation initiatives" for the 41-year Cold War, and 102 bids in the Middle East. Most came *after* a crisis of war, suggesting—as the Cuban case demonstrates—that crises and even wars may help create areas of consensus between long-term adversaries. Dramatic conciliatory gestures such as Sadat's pilgrimage to Jerusalem in 1977 also lead to major agreements. But many initiatives are stillborn. Overall, the various initiatives resulted in 29 major agreements in the U.S.-Soviet case, and 14 in the Middle East.

Kriesberg is unable to identify the causes of the successful and failed initiatives and negotiations: "no formula for peace-making efforts emerges" (79), and " . . . the use of particular negotiating tactics does not finally determine the success and failure of negotiations" (147). It is as if every bid and negotiation is unique. The various literatures that prescribe general strategies to achieve agreements are therefore of dubious validity.

Kriesberg's major contribution is to assess the consequences of agreements. Have agreements endured? Have they contributed to further moves? And, most important, is there a cumulative dynamic at work, whereby individual agreements propel a process that eventually leads to a fundamental transformation of a rivalry/conflict relationship? The methodological problems of evaluating these questions are daunting, but Kriesberg comes to the cautious conclusion that "the cumulative effects of . . . de-escalation agreements reached in previous decades greatly influenced . . . decisions on the direction of change and the resulting transformation within the Soviet Union and in Eastern

Europe [in 1989]" (191). It is too early to make a similar appraisal about the Middle East.

Whether or not limited agreements are cumulative depends very much on one's time perspective. The end of the Cold War was probably conditioned by arms control, science and technology, and cultural exchange agreements in the 1950s and 1960s. But if such agreements really mattered, how do we explain the Reagan administration's bellicosity and return to Cold War tactics in the early 1980s? If issue-specific agreements create a momentum toward a fundamental change in the conflict, momentum can also be reversed. Can we highlight limited agreements between the Cold War adversaries while playing down Gorbachev's brave decisions on Afghanistan, Germany, and on the revolutions in East Europe? These led to momentous foreign policy changes, but none resulted directly from previous formal agreements.

Kriesberg's analysis is enriched by familiarity with the historical materials, and by recognition of the important constraints—and sometimes opportunities—imposed or offered by domestic and international contexts. He emphasizes that goodwill, the search for mutual understanding, honing tactical bargaining skills, and shedding stereotypes are not sufficient steps to agreements.

About 70 percent of armed conflicts since 1945 have been civil wars or began as civil disturbances that ultimately became internationalized. Are these wars more or less intractable than long-standing rivalries and conflicts such as the Cold War and the Middle East problem? In international conflicts, competing interests are relatively easy to identify, and can often be accommodated to the satisfaction of both parties. An arms control agreement can bring benefits for both sides, for example. In civil wars, in contrast, the very existence of the state is the fundamental issue. It would be difficult to convince an Indian, Sri Lankan, Bosnian, or Cypriot that the partition of an existing state can be a win-win situation. Kriesberg's focus on just two major rivalries/conflicts thus raises the issues of comparability. It may be appropriate now for researchers to shed their preoccupation with inter-state conflicts and turn to the seemingly never-ending search for statehood among ethnic, communal, language, and religious groups. These are the main sources of conflict at the end of the millenium.

Kriesberg's value position is on the side of *peaceful* conflict resolution, although he acknowledges that some agreements may result from discrete coercive acts. He evades an implication from part of his analysis, namely that war may be a necessary condition for a permanent settlement, or that some settlements may become the prelude to future wars. The United States—like most post-1945 states—was born through military victory, not peaceful conflict resolution. What would have been the fate of the thirteen colonies—or of Algeria, Portugal's African colonies, and Vietnam, for example—had George III and his twentieth century counterparts adhered to the prescriptions of the conflict resolution literature? Ultimately, the conflict resolution literature must address the great normative issues. These concern substance, not process. What sorts of states—ethnocracies or democracies, for example—are acceptable? When is armed secession legitimate? Should the Eritreans have accepted a solution short of independence, as a compromise outcome would have demanded? Should agreements try to integrate or separate warring communities? Will an EC-UN-brokered peace in Bosnia be just? If not, it may set the stage for decades of violent revenge and score-settlement. Kriesberg has added to our knowledge of agreements and their cumulative impact, but questions about the *kinds* of agreements that issue from negotiations ultimately need to be addressed.

University of British Columbia K. J. HOLSTI

Moral Vision in International Politics: The Foreign Aid Regime, 1949–1989. By David Halloran Lumsdaine. Princeton: Princeton University Press, 1993. 355p. $49.50 cloth, $16.95 paper.

This inquiry into the evolving foreign aid policies of 18 developed democracies challenges realist theory by suggesting that ethical principles, rather than the political and economic interests of donor states, best explain foreign-aid policies. David Lumsdaine's book is distinctive and important, because it contributes to empirical theory about world politics, provides a comprehensive picture of foreign assistance in the developed democracies, and contains significant policy recommendations. Theoretically, Lumsdaine's book will take a place within a small but rapidly growing school of thought, arguing that morals *actually have shaped* the foreign policies of states. In the past few years, a number of scholars, working independently and having different but overlapping foci, have reached conclusions similar to Lumsdaine's about the role of ethics in international politics. Examples include Dorothy V. Jones's *Code of Peace* (1991), Robert W. McElroy's *Morality and American Foreign Policy*, Louis Henkin's *The Age of Rights* (1990), and Amitai Etzioni's *The Moral Dimension* (1988).

True, Lumsdaine focuses on the foreign-aid programs of developed democracies; but like these other works, his argument goes beyond the issue of foreign aid. The underlying question is whether moral vision and commitment has affected international relations since World War II; and the author's answer is a clear, unequivocal *yes*. Using a wealth of statistical evidence, the author shows that foreign aid, a postwar phenomenon, is the largest source of capital to Third World countries, far exceeding investment by multinational corporations. The conventional realist view is that the foreign-aid programs of developed countries were designed to promote the political and economic interests of donors. In contrast, Lumsdaine argues that the "principle of help to those in great need implicit in the very idea of foreign aid led to steady modification of aid practices, which focused them more on the needs of the poor and moved them away from donor interest" (p. 5).

In the process of developing this argument, Lumsdaine provides a readable, up-to-date, and accurate description of the origin and development of foreign-aid programs in the developed democracies. He also explores public attitudes toward foreign-aid programs. These features make the book worth reading for anyone interested in the foreign policies of developed democracies toward the Third World and the domestic underpinnings of those policies even if they do not believe that moral factors matter in international politics. Though Lumsdaine emphasizes the similarities in the aid programs of the developed democracies, he also offers interesting hypotheses to explain differences among their foreign economic aid efforts (as a percentage of gross national product) and in their relative emphases on targeting economic assistance to the neediest recipients. One especially provocative thesis is that the developed countries whose people support vigorous domestic antipoverty efforts also tend to support relatively large and need-targeted economic foreign aid programs. Hypotheses like this one are tested using simple statistics (no more complicated than partial correlations). Thus, in the presentation of evidence, the book strikes a nice balance between pure description and sophisticated but highly technical hypothesis testing.

Some will dismiss this book, because they will reject its main argument. For example, critics of U.S. foreign policy toward the Third World see little evidence of moral vision in it. This book will not change their minds, partly because its conclusions are based on an analysis of one relatively small part of the U.S. foreign-aid program. Critics will note that the author's argument about ever-increasing humanitarianism certainly cannot be supported if one looks at the distribution of U.S. military aid. Numerous studies have shown that there is no relationship between level of U.S. military aid and the economic needs of recipients. Neither is there much targeting of *total* U.S. economic assistance toward the neediest recipients. Economic aid channeled through the U.S. Security-Supporting Assistance (SSA) program has been used "primarily, and avowedly, to support allies and to further U.S. foreign policy goals" (p. 93). Robert F. Zimmerman makes this point clearly and effectively in his recent book, *Dollars, Diplomacy, and Dependency* (1993). Thus, a pattern of U.S. economic aid being directed to the neediest countries is only clear if one deletes the SSA portion of economic aid, which has accounted for one-third to one-half of the total since 1950. In other words, Lumsdaine's moral-vision empirical thesis is upheld only if one looks at the distribution of much less than one half of the total U.S. foreign-aid package. Lumsdaine admits this point; he does not hide the facts. Instead, he probably presents them more clearly than anyone else has.

There is room for disagreement over the interpretation of those facts, because all foreign-aid programs manifest mixed motives. The question is, How much evidence of humanitarianism is required to conclude, with Lumsdaine, that the foreign-aid programs of the developed democracies are motivated *to a considerable extent* by moral principles? For some, the evidence presented in the book will not be sufficient. What is most important, however, is that Lumsdaine is ingenious and thorough in marshaling evidence of altruism and in taking on serious counterarguments. Consider the following example. Recognizing that no foreign-aid program is moral if the standard of evaluation is absolute, he argues that the foreign aid programs of the developed democracies manifest moral vision because, *relative to other types of donors*, they are more altruistic. He makes this point by comparing the foreign-aid programs of the developed democracies with those of OPEC and CMEA countries. He shows that aid giving by DAC countries was disproportionately higher, more multilateral, and less focused on strategic allies (p. 129).

Empirical arguments about international politics are the main focus of this book; but, as you might imagine, the author's own moral admonishments also play an important part. Lumsdaine is one among many who have argued that morals *should matter* in international politics. As he makes crystal clear in his concluding chapter, "How Shall We Live?," he is an idealist, urging an even deeper commitment to moral vision in designing future foreign-aid programs. He believes that peace and prosperity can only be sustained within a just international order—that renewed moral vision is a prerequisite to developing workable institutions for a post–Cold War world.

Binghamton University DAVID LOUIS CINGRANELLI

Coercive Cooperation: Explaining Multilateral Economic Sanctions. By Lisa L. Martin. Princeton: Princeton University Press, 1992. 299p. $39.50.

Most studies of economic sanctions have focused on the question whether sanctions work as an instrument of coercion. Fewer studies have systematically explored one of the prerequisites for successful sanctions—the extensiveness of multilateral cooperation. This book attempts to fill that gap by integrating theories of cooperation with the study of economic statecraft. In so doing, it makes a number of contributions to our understanding of multilateral sanctions; and its conclusions provide some useful guidance to practitioners.

In seeking to determine the conditions conductive to multilateral sanctions, Martin employs a multimethod approach. In part 1, she uses game theory to develop a useful typology of cooperation problems: (1) *coincidence*, where common interests naturally lead to cooperation; (2) *coercion*, where divergent interests require the leading sender to use threats and promises to alter the preferences of others; and (3) *coadjustment*, where the mix of convergent and divergent interests requires coordination. These models are then used to generate hypotheses, which are tested through three quantitative methodologies (regression, ordered probit, and events data analysis) using 99 cases drawn from Hufbauer, Schott, and Elliot's *Economic Sanctions Reconsidered* (1990). Part 2 traces the causal relationships suggested by the data analysis through four case studies in order to elaborate on, and refine, the original hypotheses.

In addressing theories of cooperation, Martin sidesteps debates between neoliberals and neorealists by arguing that each is correct "given certain configurations of state interests" (p. 30). Nonetheless, the statistical analysis calls into question neorealist assumptions that institutions are passive reflections of state interests by finding that the use of institutions, rather than hegemonic power, had the strongest impact on cooperation. In fact, the results of the data analysis confound the expectations of the hegemonic stability model in two ways: (1) the United States has registered less cooperation than other sanctions initiators, and (2) its ability to secure cooperation has increased over time, even though its economic preponderance has diminished (pp. 86–90).

The case studies on European Community sanctions against Argentina during the Falklands War and the CoCom strategic embargo against the USSR lend further support to neoliberal arguments regarding institutions and cooperation. First, in both cases, institutions helped overcome collective action problems by providing a formal means to reaching consensus and by dampening the fears of exploitation. Second, institutions provided a bargaining arena where linkage to other issues could more effectively be used to induce cooperation (e.g., Britain's concessions on European Community budget and agricultural policies in exchange for sanctions). Finally, participation in institutions can influence how states define their interests, thereby discouraging noncooperation in ways that might risk the loss of services provided by those institutions (p. 245). For example, the fear of losing European Community benefits induced a number of smaller European states, particularly Ireland, reluctantly to agree to sanctions as an alternative to war and to maintain sanctions even when hostilities broke out.

The other factor that was strongly associated with successful cooperation was the willingness of the main sender to absorb major costs in its choice of sanctions instruments. This outcome was associated with credible leadership, because it legitimized collective efforts, demonstrated the initiator's ability to overcome domestic opposition, and made threats and promises to other potential sanctioners more persuasive (p. 244). The most telling example cited was Carter's imposition, and Reagan's repeal, of the grain embargo. The former's willingness to take costly actions enhanced his ability to negotiate stronger technology controls within CoCom (the "no-exceptions" policy). The latter's refusal to bear costs undermined his credibility in forging cooperation after the Polish crisis (pp. 234–39). While neorealists also emphasize leadership, they tend to define it in terms of hegemonic coercion or side-payments. They are skeptical of nonhegemonic cooperation, even if there are convergent interests (coadjustment games), because considerations of "relative position" generally take precedence over common interests. Indeed, in several cases that Martin cites, a significant impediment to cooperation was the skepticism that the lead sender's ulterior motive was to improve its commercial position vis-à-vis competitors. A contribution of Martin's study is that even under conditions of declining hegemony, the willingness of the lead state to pay a real price in implementing sanctions can alleviate many of the suspicions that deter collective action.

At the same time, however, some of the evidence seems to be strained to fit the thesis. For example, Martin contends that the Reagan administration's unwillingness to bear costs was primarily responsible for the intensity of European opposition to the pipeline sanctions (p. 239). She also argues that the central difference between Carter's failures to coordinate sanctions against human rights violations in Latin America (p. 128) or the Soviet Union (p. 200), in contrast to his success with the "no exceptions" policy, was the absence of institutions and self-imposed costs. But there was another difference between these successes and failures. The former, in denying dual-use technologies to the USSR, represented an area where the United States wanted to tighten a policy within an existing consensus. The latter (economic warfare against the USSR or tactical linkage to human rights) were issue areas where no such consensus existed. In other words, the key determinants of successful cooperation may be whether the interaction resembles coadjustment or coercion; and the explanatory variables cited by the study may apply only to coadjustment.

To test this conditional version of Martin's thesis, the data analysis should have been disaggregated to test coercion and coadjustment scenarios separately. While the author notes the difficulty in constructing such an exercise (p. 44), systematically choosing case studies that resemble each model might have provided more insight as to when leadership and self-imposed costs can be successful. Such an analysis might also have provided a more sophisticated critique of the hegemonic model, which sees economic dominance as necessary for cooperation in both coercion and coadjustment games. Martin convincingly uses the case studies to challenge the hegemonic model for coadjustment, yet a more differentiated analysis would be necessary to refute it for coercion.

Despite these reservations, this is an important book that ought to be consulted by all serious students of international cooperation and economic statecraft. It develops conceptual refinements of cooperation dilemmas and suggests interesting relationships that ought to be explored in future research. And as multilateral sanctions and export control regimes play a more prominent role in a post–Cold War order, practitioners should absorb the central lessons of this book, namely, that institutions matter and that leadership cannot be exercised "on the cheap."

Colby College KENNETH A. RODMAN

Economic Containment: CoCom and the Politics of East-West Trade. By Michael Mastanduno. Ithaca: Cornell University Press, 1992. 353p. $46.50 cloth, $18.95 paper.

In the post-Cold War world, both U.S. and multilateral export control policy has come into increasing disarray. The revelations that the U.S. approved the sale of embargoed items to Iraq before the Gulf War and then subsequently covered up the sales is only the most recent example. In this excellent book Michael Mastanduno probes the sources of these and other problems plaguing Western efforts to control the diffusion of goods and technology to potential adversaries. He analyzes the roots of export control cooperation and conflict in U.S. Cold War trade policy and alliance politics. In doing so, he also makes an important contribution to the literature on international cooperation and U.S. foreign economic policy. Through a careful empirical examination of the evolution of export control policy this book explores the more general role of hegemonic leadership in maintaining cooperation among states, the role that domestic politics plays in strengthening or weakening the leadership position, and the role that international institutions can play in shaping domestic policy.

Building on and refining hegemonic stability theory, Mastanduno argues that an effective Western embargo required a leader, a state that would become the "benevolent" hegemon in CoCom (Coordinating Committee for Export Controls), the multilateral embargo regime. Membership in CoCom largely corresponded with membership in the NATO alliance. In the post-war world of American dominance, that leadership role fell to the United States. Leadership was needed, Mastanduno argues, because CoCom members had divergent and sometimes conflicting preferences. The United States preferred a strict and sometimes wide embargo. Since it maintained a more confrontational relationship with the Soviet Union and had to assume responsibility for West Europe's defense, the United States had the most interest in maintaining military lead-time over the Soviet adversary. One way to do this, it was widely believed, was to ban the export of a wide range of "militarily significant" goods and technology and, at times, to influence Soviet foreign and domestic policy by increasing or withholding trade. Other CoCom members supported a narrow strategic embargo, but their preferences for a less confrontational relationship with the Soviet Union and their economic interest in East-West trade prevented them from supporting more all-encompassing restrictions and from encouraging the use of economic sanctions as a foreign policy tool.

The preferences of CoCom members overlapped to the extent that the Western powers were able to establish the regime as a multilateral institution to limit trade in goods that would contribute to Soviet and Warsaw Pact military power. But conflicting preferences over the appropriate multilateral strategy for restricting trade persisted. For a common export control strategy to emerge and for CoCom to be effective, Mastanduno argues, a leader was required to maintain the integrity of the control process, obtain the compliance of non-CoCom suppliers with CoCom rules, set a domestic example, and minimize the administrative and economic burdens of the control system. The logic of the argument to this point largely explains the tenacious conflict in CoCom over appropriate strategy and the problematic role of U.S. leadership; the leader's preferences were essentially at odds with those of the followers. The requirement for institutional leadership often conflicted with the structural interests of the United States. Even though the U.S. possessed the structural capabilities required for leadership, that is, preponderant—albeit declining—control over power resources, the leadership position was bound to be a problematic one for the U.S. in an institution where consensus and compromise were the dominant decision modes. Indeed, the U.S. proved to be an ineffective and inconsistent leader in CoCom for much of the Cold War period. The empirical evidence points to the conclusion that, with the exception of the Korean War period, only when the leader's preferences fell in line with those of the followers, and only when all preferred a strategic embargo over economic warfare or various linkage strategies, could leadership be exercised and cooperation assured. This convergence of preferences was rare during the Cold War, suggesting that the U.S. was a weak leader indeed.

Why? Mastanduno demonstrates that the leadership performance of the U.S. did not correlate with the relative decline in U.S. economic power. He therefore looks for the explanation at the domestic level. He points to an extremely powerful but fragmented American state. At first glance this explanation is counterintuitive, because with regard to export control, the American state—that is, the executive branch of government—was strong. It was well insulated from the private sector, and the private sector was not well organized to oppose export control. But the executive was often divided between forces that opposed extensive controls and those that supported them. The combination of state strength and internal fragmentation led to policy patterns that were sometimes at odds with the U.S. leadership role in CoCom. Often when the state was *strong*, that is, when the forces of the American state were arrayed on the side of export controls as a foreign policy instrument, U.S. policy came into intense conflict with the policies of other CoCom countries; the United States attempted to use CoCom as a multilateral sanctions tool—a purpose for which it was not intended—therefore compromising the integrity of the control process. In these cases, the U.S. projected its own domestic export control policy onto the multilateral arena, a policy that was insensitive to the economic costs that broad controls would impose on America's allies. In attempting to gain compliance, the U.S. would often use extraterritorial controls to coerce allies to conform to its preferences, further alienating its CoCom partners. Under such conditions, the allies balked or rebelled, and U.S. leadership failed. When the American state was *fragmented* in its policy preferences, policy was inconsistent and unpredictable; again the United States could not preserve the integrity of the multilateral control process because the Executive branch was deeply divided internally over policy and, as a result, conveyed conflicting messages to CoCom allies. Domestic squabbling undermined the ability to exercise international leadership.

Not only does Mastanduno examine how U.S. domestic political structure influenced its leadership role in CoCom, but he also looks at how the CoCom regime influenced U.S. trade policy. He argues that participation in CoCom strengthened the hand of economic liberals over economic nationalists in the U.S. government. CoCom, he suggests, served as a "firebreak" to the spread of economic nationalism in that it protected the liberal orientation of U.S. trade with Western states from contamination by protectionism inherent in America's control program. Economic nationalists could focus their energies on restricting trade with adversaries, and export control policy drew attention away from those forces that were inclined to restrict economic relations with trading partners. The evidence suggests that the stronger CoCom has been, the less pressure has arisen in domestic politics for the United States to expand intra-western restrictions. When the effectiveness of CoCom weakened, U.S. officials were tempted to blur the distinction between East-West trade and trade between the United States and the rest of the world.

Mastanduno tests this argument by comparing the strength of multilateral cooperative efforts in four periods of CoCom's existence: the period of the early Cold War in which tension between the U.S. and the Soviet Union was quite high; the decade of the 1960s in which U.S.-Soviet tension remained high; the 1970s period of détente and after; and the 1980s, in which West Europe wished to relax tensions and the United States did not. The research design is carefully constructed as a "crucial case study" to compare changing preferences of the U.S. and its allies, changing and divergent domestic preferences within the executive branch of the American government, changing international conditions, and changing U.S. trade policy. For each period Mastanduno isolates the factors that contributed to cooperation and the factors that contributed to its decline.

Mastanduno's findings support the overall argument. The evidence reveals variations in the strength of cooperation within CoCom that are linked to the consistency and effectiveness of U.S. leadership and to the convergence or divergence of preferences among members. Before 1954, cooperation in CoCom was strong, because preferences more or less converged due to the Korean War and the fear of a Soviet invasion, and the executive branch of the U.S. government was unified on export control goals. Between 1955–57, CoCom was relatively ineffective because member governments were locked in a policy conflict over China, which led some to demand exceptions to the embargo, and others to openly defy the embargo and to question the continued viability of the regime. The United States could not effectively lead because it insisted on its preference for economic warfare against China, but could not give a *strategic* rationale for those preferences. But between 1958–68, friction in CoCom was minimized because domestic forces ensured that U.S. policy was consistent with a "narrowly circumscribed, uniformly applied strategic embargo." Between 1969 and 1979 leadership was again ineffective and levels of friction high because national security export controls were undermined by attempts to pursue the political and economic objectives of a fragmented state. During the 1980s the picture was mixed; there were improvements in some aspects of cooperation, but U.S. failure to decontrol less strategic items caused increasing friction within CoCom.

Economic Containment is an extremely well-developed argument about the domestic sources of international cooperation and the international constraints on domestic policy. Mastanduno does not pretend to generalize the argument beyond the issue of Western multilateral export control and takes care not to overstate his case. Nonetheless, testing the overall argument in other cases under different conditions, modifying and revising its claims based on new evidence will be an important area for future research.

Nonetheless, the explanation for variations in cooperation in this case may be somewhat overdetermined. Leadership in CoCom was certainly required when strategies of economic warfare and tactical or structural linkage were attempted. But, in large part, leadership failed when these strategies were at odds with the preferences of America's European allies. Only when preferences on a strategic embargo converged, was leadership successful. Therefore, good leadership corresponds closely to convergence on *specific* preferences. Domestic level events may explain the origin of preferences more clearly than they explain leadership ability.

A second issue concerns Mastanduno's two-fold definition of CoCom's effectiveness. Effectiveness in his terms means both the embargo's influence in preserving U.S. military lead-time over the Soviet Union *and* high levels of multilateral cooperation. The book focuses primarily on this second aspect of effectiveness, and, as discussed above, the argument is convincing. However, little time is devoted to the first aspect of effectiveness, and the evidence for the arguments with regard to the importance of the strategic embargo in preserving U.S. military lead-time is somewhat thin. For example, Mastanduno cites the Toshiba-Kongsberg case (in which Toshiba Corporation illegally sold submarine-quieting technology to the USSR) as evidence that the Soviets had made significant progress in some areas in reducing U.S. lead-time through illegal acquisitions during the period of CoCom weakness in the 1970s and early 1980s. There is little evidence, however, in this particular case, that the Soviets made any progress at all with Western technology. Pentagon officials never confirmed whether the Toshiba technology had actually been installed in Soviet submarines. Most Pentagon planners admitted that the Soviets had not had enough time to actually put the technology to use, and military analysts in the U.S. admitted that most of the Soviet improvements in submarine-quieting technology were the result of long-term, evolutionary, broad-based engineering and scientific development efforts. Toshiba was singled out to take the blame for what was really a long-term, overall decline of U.S. superiority in submarine-quieting technology. Mastanduno neglects to consider that weak and non-existent evidence concerning the impact of Soviet acquisition of Western technology on Soviet military strength may have played a role in the disputes between the U.S. and other CoCom members; the U.S. often exaggerated the claim that without a wide embargo, Western security was endangered. Lack of evidence for those claims undermined them in the eyes of Europeans, and thus undermined possible consensus on items that needed to be controlled.

But this is simply a minor complaint. *Economic Containment* is the definitive study of CoCom to date, and as such it sheds important light on the origins of international institutions, why they survive and why they might perish. It is one of the most important recent books on international cooperation and the impact of multilateral institutions on U.S. foreign economic policy. Mastanduno has established both empirical and conceptual links between international and domestic structures and processes, and in doing so he has taken a major step in improving our understanding of both multilateral cooperation and the international institutional constraints on domestic policy choice.

University of California, Berkeley BEVERLY CRAWFORD

International Public Finance: A New Perspective on Global Relations. By Ruben P. Mendez. New York: Oxford University Press, 1992. 339p. $45.00 cloth, $24.95 paper.

The end of the Cold War has dramatically changed the agenda of international politics. With the virtual disappearance of a security dimension to many inter-state relations, such topics as the environment, immigration, public health, and minority rights have become more prominent concerns of national foreign policies and international institutions.

One interpretation of this trend is that national security-related problems are fading in importance relative to global concerns. While the world's major states used to focus on securing military cooperation among allies and vigilance against enemies, today there is more scope for a global search to cooperate against evils that might be regarded as the enemies of humanity in general. Redoubled attention to trans-border pollution, global warming, and human rights may presage the development of inter-state cooperation to improve *global* welfare.

International Public Finance takes this interpretation to perhaps its most ambitious—and perhaps most optimistic—conclusion. It supposes that, just as national systems provide a variety of public goods to their citizens, so too can the international community provide global public goods to the citizens of the world. Ruben Mendez presents both a positive analysis of the Economics of such global provision of global public goods, and a normative argument for its feasibility, with a strong emphasis on the problems of financing such arrangements.

Mendez begins with a useful summary of the Economics of national public finance. His principal purpose is to explain the conditions under which government intervention is justified to provide goods and services that would otherwise go unprovided or under-provided by the private sector. In this Mendez emphasizes market failures due to incomplete information or imperfect competition; the need for an authoritative agent to overcome collective action problems in the provision of public goods; and the importance of government policy in the mitigation (encouragement) of activities with negative (positive) external economies.

The second part of *International Public Finance* describes the state of contemporary international organizations associated, even if only peripherally, with the provision of international public goods. This consists of a historical account and contemporary survey of which financial resources international agencies (and bilateral aid-granting sources) have at their disposal and how they are disbursed.

In the third section of the book, Mendez moves to his proposals for a series of international initiatives to finance the provision of global public goods. He is especially concerned to build into international public finance the automaticity present in virtually all national public finance. This would reduce or eliminate the financial reliance of international endeavors on voluntary national contributions, and would replace or supplement voluntarism with automatic revenue sources.

In this context, Mendez presents a large number of proposals for automatic international revenue generation. Some of the proposals are straightforward: a small tax on international trade or shipping, compensation to sending countries for the "brain drain." Others are more imaginative: charges for use of such global commons as Antarctica, geostationary orbits, and the electromagnetic spectrum, or a United Nations lottery.

As perhaps befits a long-time employee of the United Nations specializing in economic and financial issues, Mendez evinces frustration at the state of current international organization. However, he also demonstrates a refreshing optimism about the prospects of adoption of new international financial mechanisms that will allow for the more efficient provision of goods and services he regards as the proper province of international agencies.

International Public Finance is indeed a well-informed, intelligent treatment of the problems and prospects of international cooperation, especially the financing of multilateral organizations. There are nonetheless some gaps in the treatment Mendez accords to the issue. He is quicker to see market failures than most analysts, perhaps in his zeal to argue the need for an international agent to overcome such failures. Parallel to Mendez's tendency to overestimate the scope for global action is his inclination to underestimate the political and other problems associated with securing such action.

Indeed, the book suffers from inadequate theoretical and empirical attention to the problems of international cooperation. There is by now a large literature on conditions likely to promote or impede inter-state agreement on the provision of what might be regarded as international public goods, from monetary stability and the protection of contracts through the environment. There are also many studies about contemporary and historical experience with attempts at such cooperative endeavors. Inasmuch as the principal reaction of most readers to *International Public Finance* will be to call into question the feasibility of its proposals, Mendez would have served his cause better with a careful analysis of the socio-economic and political conditions that affect the probability of adoption of international cooperative financial schemes.

There are indeed plenty of possible normative arguments for higher levels of inter-state cooperation on the environment, development, public health, and macroeconomic issues. However, those who present such arguments have something of an obligation to discuss the circumstances under which such cooperation is likely to be feasible. Mendez is clear and forthright in presenting his case for the global provision of global public goods, but he does not sufficiently explore the conditions under which such advances might take place. This is a problem even for such true believers as Mendez, for without a clear picture of the arenas in which success is more likely it is hard to know where international reformers would want to begin their efforts.

Mendez also demonstrates a modest bias toward analytical and normative conclusions that favor the developing countries. For example, he is quite tolerant of imperfect competition when the cartel in question is made up of commodity producers from the developing world, such as OPEC. He argues that redistribution at the global level is more pressing than domestic redistribution, although this conclusion would be challenged by many who think the principal solution to poverty in the developing world is equitable economic growth (leaving aside the feasibility of large-scale international redistribution). And Mendez sometimes expresses dismay at the pursuit by developed countries of their perceived national economic interests, even while he accepts uncritically analogous pursuits by developing countries.

These concerns aside, *International Public Finance* is an illuminating, forward-looking book. Ruben Mendez has had the imagination and foresight to hope that the future will bring increased interest in international cooperation on a wide range of issues. More, he has thought long and hard about ways in which a world more concerned about global problems might organize itself to tackle them. Ten years ago, the arguments Mendez presents might have appeared eccentric; today they seem only visionary, and they may indeed be prophetic.

University of California, Los Angeles JEFFRY A. FRIEDEN

Principled Diplomacy: Security and Rights in U.S. Foreign Policy. By Cathal J. Nolan. Westport: Greenwood, 1993. 292p. $55.00.

Cathal J. Nolan ends this provocative study with two points that should have been in the first chapter. He finally tells the reader then (although it has long been evident) that he tends "to work out of the realist analytical tradition" (p. 267). Thus, it is not surprising that he shows considerable sympathy for executive officials like Richard Nixon and Henry Kissinger and others who sacrificed various human rights concerns for the "preservation of the basic economic and political infrastructure of the community of democratic nations" (p. 268). He also finally makes clear that, like Reinhold Niebuhr, he is interested in subject matter "where ethics and power combine in a vortex of compromise and moral ambiguity" (p. 271). Hence, the author presents a study of the place of human rights policy as part of the larger subject matter of U.S. foreign policy in a world of powerful and supposedly sovereign states. To do this, he concentrates on two dimensions of U.S. policy: bilateral relations with the Soviet Union, and multilateral relations through the United Nations.

His dominant theme is that "as a consequence of U.S.

diplomacy, ideas about representation, liberty, and a direct connection between human rights and international security have become increasingly important in defining and perhaps sustaining world order in the modern era" (p. 2). A subsidiary theme is that this outcome was more the work of the executive, than the legislative, branch. When referring to Congress and human rights policy, Nolan uses words like "distorted" and "myopia" (pp. 3, 269).

Parts of his analysis support this interpretation. For example, chapter 7 on the Truman administration and the introduction of human rights language in the United Nations Charter is closely researched and reasoned. He largely makes his case that the Truman administration should be given primary credit for these developments—not domestic American nongovernmental organizations or certain hemispheric states. This is a new understanding, superseding other interpretations on which many had relied. Of course, key members of Congress were also involved in the fashioning of the Charter (as he acknowledges but does not emphasize); and the Senate gave its advice and consent overwhelmingly.

Other parts of his analysis are equally persuasive. For example, in chapter 3 he notes that the United States engaged in the forced repatriation of Soviet prisoners of war toward the end of World War II, in pursuit of the goals of speeding the return of American prisoners of war in Soviet hands after their liberation from German camps and of continuing broad cooperation with Stalin. This does, indeed, show the collision of ethics and power considerations in a "vortex of moral ambiguity." Also illuminating was his review of American humanitarian assistance during and after the Russian Civil War.

Nolan is on safe ground (as others have shown) in observing the damage done to U.S. policy by the Bricker movement in Congress, although Brickerites were motivated by more than isolationism. There is no mention, in his account, of Southern racists who opposed international human rights treaties and who supported the generic Bricker amendment for racist reasons, hiding behind defense of states rights. Other members of Congress supported Bricker out of genuine concern for congressional authority in the face of expanding executive power. There was much more behind Brickerism than isolationism.

He is likewise on solid ground in presenting Senator Henry Jackson's use of emigration rights as a weapon to disrupt détente with the Soviet-led communist camp, although he understates the number of members of Congress who supported emigration and other rights in communist states for other than hawkish reasons. Nolan's understanding of Congress and human rights tends to be one-dimensional: he sees the problems but not the contributions.

In my judgment, Nolan overstates his case because of both method and subjectivity. As for method, in looking only at Soviet and United Nations affairs, Nolan screens out many factors that do not support his emphasis on American "good works" (my term) or executive primacy in support for human rights. Had he focused on U.S. policy toward developing nations, he might have abandoned his twin theses as characteristic of U.S. policy in general. Was active executive suppression of human rights in places like Guatemala after 1954, Brazil in the 1960s, or Chile from 1973 conducive to defining or sustaining human rights in world affairs? Hardly. Has the executive played a consistently leading role on human rights through the Organization of American States? Definitely not. Did the executive prop up various repressive governments, as in Zaire? Of course. Did President Reagan lag behind congressional elements in dealing with human rights violations under Ferdinand Marcos? Yes. Was the congressional role for human rights so ill considered or secondary concerning South Africa? Not at all. Did the Congress compel the State Department to pay more serious attention to human rights reporting around the world? Absolutely.

As for subjectivity (with due regard for the elusiveness of that term), it comes through most clearly with regard to Nolan's semantics pertaining to Congress. Literature that does not support his view is referred to repeatedly as "narrow" (e.g., pp. 209, 227). Authors who support his view that President Eisenhower was correct to sacrifice "minor" United Nations human rights treaties so as to defeat the congressional Bricker movement are referred to as "respected" (pp. 224, 236). Literature that views favorably congressional efforts to check Reagan's crusading American exceptionalism is called "bitter" and "hostile" (pp. 249, 259), although the critiques of executive policy are not refuted.

And to present both Nixon and Kissinger as more seriously interested in international human rights than the Congress in the mid-1970s certainly struck me as a subjective and unpersuasive argument. After all, the human rights component of the Helsinki Accord was the product of West European, not American, diplomacy. It succeeded in important ways despite the clear opposition of Kissinger and because of strong support from Congress.

Nevertheless, because of its attention to the complexity inherent in ethical choice in an anarchical international society where security policy is trumps and because of its attention to congressional-executive interaction, this study of U.S. human rights policy stimulates even if it does not always convince. The second half of the book on UN affairs is particularly well structured.

University of Nebraska, Lincoln DAVID P. FORSYTHE

The Strategic Defense Initiative. By Edward Reiss. New York: Cambridge University Press, 1992. 249p. $49.95.

Any scholar who seeks a comprehensive and reasonably parsimonious explanation for the Strategic Defense Initiative (SDI) faces a daunting task. The SDI appeared more or less out of nowhere, quickly developed into a multi-billion-dollar effort even though many influential policymakers (and almost every independent scientist) considered the idea preposterous, and is rightly characterized as perhaps the most controversial strategic program ever. It faded rapidly after 1988, however; and despite efforts by supporters to promote scaled-down versions, the space-based component was effectively abandoned in 1993. Transformations in the SDI program took place alongside huge shifts in international politics. Such a large degree of variation in both dependent and independent variables poses a challenge for researchers trying to connect cause and effect.

Reiss recognizes the scale of his task as he attempts to explain in comprehensive fashion the history of the SDI program. His goal is to explore "the *interaction* of factors strategic, political, economic, technological, institutional and cultural" as they affected SDI (p. 1), rather than limit himself to one or two explanatory frameworks. Reiss sets out a number of competing explanations for the rise and consolidation of SDI. The major ones are a rational actor–strategic model, which posits that SDI was a sensible replacement for Mutual Assured Destruction and a prudent hedge against Soviet technological breakout; a bureaucratic politics–interest group model (a variant of the military-industrial-complex thesis) in which players saw opportunities for empire building and corporate enrichment; an arms-economy model, in which SDI would channel federal funds to high-tech industries and soak up excess demand in a capitalist economy (the Baran–Sweezy conception of defense spending); and a psychological–cultural model, which emphasizes the differences between Americans and "communists" and relies on the potent symbols of threats and opponents to justify new weapons programs. Reiss finds that all can be used to explain one facet or another of SDI.

Reiss summarizes a wide range of material and offers a number of interesting speculative discussions of some nontraditional interpretations of SDI. At one point, he even plays with the idea of deconstructing the "binary opposition of supply and demand" as it affects the arms race (p. 19). The discussion of advisory panels and other low-visibility organizational dynamics is illuminating, as is the chapter on the cultural aspects of SDI. Reiss also offers valuable insight to how Europeans viewed SDI, noting that to many Europeans, SDI seemed to be what German defense minister Manfred Worner dismissed as "Zukunftmusik" (pie in the sky, p. 125).

The breadth of the approach—initially a source of strength—eventually leads to some weaknesses. The major problem is that Reiss's effort to consider as many explanations as possible leads to a disjointed and inconclusive theoretical discussion. Though Reiss says he will explain why SDI developed as it did in the face of clear evidence that it would not work and near-unanimous opposition by nongovernmental scientists, he does not assess the competing explanations or identify the explanatory model or models that he finds most persuasive. In his conclusion, he casts a very wide net, attributing SDI's rise to "technological progress, the climate of superpower hostility, ideological antagonism, action/reaction, 'hedging' against the enemy, institutional incrementalism, bureaucratic inertia, the profit motive and coincidence of interest" (pp. 195–96). Yet there is no integrated theoretical framework or synthesis that would allow the reader to place these explanations in context.

Anyone who purported to offer a unified theory of SDI would undoubtedly oversimplify; but Reiss ultimately goes too far in the other direction, arguing that the complexity of the program makes generalization impossible. Efforts to explain SDI, he concludes, are destined to fail because "the causes assigned to SDI depend on how we place it in context—its role in and influence on everything else" (p. 202). Such attempts, Reiss argues, would be as futile as looking for a single cause of World War II. This, I think, reflects too limited a perspective about the nature of social science research. Scholars need not focus exclusively on complete and singular explanations to gain insight into significant phenomena. We are also in the business of identifying and classifying midrange explanations and specifying the conditions in which one or another explanation is more powerful than its alternatives. Yet Reiss appears to move directly from the observation that a unified explanation of SDI is an impossibility to the conclusion that it is therefore pointless to discriminate among any of the potential candidates. This is unfortunate, since the theoretically appropriate task is not simply to select either a single explanation or "all of the above" but, rather, a matter of ranking the possibilities in terms of their power and showing why and how that power varies. At times, Reiss does note that different theoretical approaches can explain different aspects of SDI; but he fails to integrate the pieces of the argument and eventually, in his conclusion, rejects this approach. Reiss's reluctance to commit is clearest in the book's final passage. Here he concludes that SDI will someday be seen as either an example of what is best about democratic politics or as "an evil past imagination." Surprisingly, Reiss does not say which position he thinks is correct (and both cannot be true): "Which of the available perspectives has most validity and what new prospects will unfold, history will tell" (p. 202). Many readers may find this indeterminacy frustrating.

A related problem is that the lack of a clear explanation for SDI's rise leads to difficulties in accounting for SDI's *fall*, as well. Even though there is no explicit acknowledgment, readers will likely infer that Reiss favors the bureaucratic politics–interest group model. He argues that "the key to the continued success and development of SDI was the role of powerful groups which gathered around the programme" and that "contractors had at least some success in buying support for the SDI programme" in Congress and cites Hans Bethe that a weapon system as large as SDI will "acquire a life of its own [and] become an unstoppable juggernaut" (pp. 68, 108, 111). Yet Reiss's reliance on this model leaves important questions unanswered. If SDI contracts were so valuable to the scientific community, why did seven thousand academic scientists and engineers pledge not to accept SDI funds (p. 89)? If the military, contractors, and other interest groups were so powerful, why did Congress cut the SDI budget request every year and cut total spending after 1990? If legislators had an explicit stake in SDI and answered to powerful interests who wanted the program to continue, why did they block a proposed "independent" SDI think tank, prevent the Reagan administration from reinterpreting the antiballistic missile treaty, and refuse to fund early deployment? Bethe's observation is simply not borne out. Although Reiss attributes these defeats to the power of congressional liberals and the arms control lobby, his account does not help us understand why SDI opponents won here, but not elsewhere in the program.

Reiss offers a number of interesting interpretations of SDI, and those who seek a broader perspective on the program will find the book useful. As a mainstream history and explanation, however, the book provides too little theoretical context and resolution. In the attempt to explain *everything* about SDI and utilize every extant theoretical framework, Reiss does not adequately distinguish the critical from the incidental. A more focused approach that explicitly considered a smaller number of hypotheses in more detail and attempted to evaluate the competing models would have been more valuable.

University of Wisconsin, Madison KENNETH R. MAYER

The Politics of Trade in Latin American Development. By Steven E. Sanderson. Stanford: Stanford University Press, 1992. 292p. $45.00 cloth, $16.95 paper.

This volume explores a variety of issues connected with trade and development in Latin America, including the impact of trade on development, the impact of trade on the environment, opportunities and constraints facing small and large Latin American states in confronting international trade, and the effectiveness of alternative trade and development strategies in producing improved welfare and income distributions.

Sanderson is skeptical of increased trade as a development strategy in Latin America, both because countries face various barriers to increasing exports, and because past experiences of export-led growth (much less export-led development) in the region have been disappointing. The book is laced with critiques, often very effective, of the assumptions of free-trade ideologues. Sanderson also sometimes challenges aspects of economic nationalism and anti-trade literatures, such as the concept of deteriorating terms of trade. But for the most part the book is a response to the inappropriate optimism of multilateral organizations and the OECD countries for trade liberalization as a path toward growth and development in Latin America.

The Politics of Trade also underscores contradictions in the developed world prescriptions, for example that Latin American countries export their way out of their debt crises while at the same time protecting their natural environments. Indeed, Sanderson's discussion of the relationship between trade and environmental exploitation is one of the most provocative parts of the book, as well as one of the grimmest. He suggests that international pressure to privatize will only make for greater environmental degradation, since the market has no mandate for environmental protection whereas the state potentially does (although he also argues that the Latin American state "as presently constituted cannot be expected to challenge environmental degradation" [p. 84]).

The volume contains a very rich bibliography reflecting its authors' immersion in several literatures.

Still, upon finishing this book I felt like someone who has gone to a dinner hungry but is served only an appetizer. One source of dissatisfaction is that, the book's title notwithstanding, there is very little political analysis in it. Sanderson makes the point repeatedly that international trade involves powerful nations influencing both the external and domestic policies of weaker, dependent ones, a proposition that is certainly true. But aside from observations about growing U.S. protectionism and the Uruguay round of the GATT negotiations, that is as far as the analysis of the international politics of trade goes. One has the sense that the unequal power of actors in the international trade system is what Sanderson means by the "politics of trade"; and he delves no farther.

The domestic politics of trade within Latin American nations is also not as thoroughly explored as one might have hoped. Sanderson presents hypotheses and data concerning the relationship between the strength of the public sector and economic openness: he proposes that stronger states are better than weaker ones at promoting exports. (Sanderson says that the data particularly on state strength are fragmentary; although the hypothesis is not definitively proved to be true, it remains a provocative idea).

So far, so good. But there are many questions that could be addressed that would tell us much more about the domestic politics of trade in Latin America. Have Latin America's bureaucratic-authoritarian versus civilian regimes demonstrated any systematic differences in trade policies? (A decade ago any Latin Americanist would have answered yes; now the answer is not so clear.) Are different kinds of regimes more or less effective in confronting the international market? Does concentration on distinct kinds of commodities—say, traditional agricultural exports versus more value-added exports—favor different economic sectors and thus change the balance of power in domestic politics? In the recent period, is there a "bureaucratic politics" of trade, with some Latin American political institutions favoring and others opposing particular trade policies?

The answers to these kinds of questions would provide us with a much deeper understanding of the politics of trade in Latin America. Their absence lessens the book's contribution, in comparison to works on related topics, such as John Sheahan's *Patterns of Development in Latin America* (1987), or Barbara Stallings's *Banker to the Third World* (1987).

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The Suffering Grass: Superpowers and Regional Conflict in Southern Africa and the Caribbean. Edited by Thomas G. Weiss and James G. Blight. Boulder: Lynne Rienner, 1992. 182p. $30.00.

Ever since the late 1950s, students of regional international politics have attempted to stimulate interest in comparative regional system analysis. The basic idea has always been that there is more to international politics than the global contests of the strongest states. A number of geographical niches have developed subsystemic patterns of interaction, with varying degrees of autonomy, into which major powers may intervene but not always predictably. Global rules of engagement and hierarchy do not always work the same way in the different regions of the world. As a consequence, major power attempts at intervention are often less than successful because the appropriate decision makers may not realize or fully appreciate that they are no longer playing in the global game and are not quite sure what the game rules or even the stakes might be in various regional games.

One likely outcome of this state of affairs is that major-power attempts to reduce regional conflict may backfire and, instead, work to exacerbate tensions and hostilities within regional systems. Especially within the context of an ideological Cold War and two superpowers with zero-sum perspectives on who—between the rival bipolar nodes of the global system—is ahead in any given region at any given point in time. But what happens to regional international politics in the aftermath of the Cold War? Will regional actors be free of the Washington–Moscow temptation to interpret all conflicts through superpower lenses? And if this outcome is possible, will regional actors be any better off when they are ignored than when they were given too much attention?

These are the sorts of issues raised by Weiss and

Blight, who have edited a collection of case studies on recent American/Soviet/Cuban/South African behavior in the regions of Southern Africa and the Caribbean Basin. Three questions were supposed to provide cohesion for the case studies: Have superpower policies worked to increase or decrease regional conflict? What role do regional hegemons play? and What outcomes should be expected for third-party diplomacy? Six authors were asked to address these questions for their respective regions in a before- and after-Cold War context.

Case studies that hope to generate comparative and theoretical payoffs are quite difficult to execute. While all of the case analyses are definitely worth reading, it is primarily for things other than explicit comparison and theory that this book's chapters are useful. For example, Jorge Dominguez' chapter on Cuba is a quick read on Cuban grand strategy. But in general, the descriptive chapters are just that and the editors' preface and conclusions are unable to transform the material into something other than useful overviews of recent regional politics.

What went wrong on the road to comparison? Several possibilities seem pertinent, and none are mutually exclusive. First, the editors acknowledge that the disintegration of the Soviet Union took place while their volume was in preparation. What had begun as an analysis of the comparative effects of the regional policies of the superpowers turned into an inquiry about what might happen when only one superpower was left in the arena. The volume's initial purpose was thus overtaken by current events—a fate to which comparative regional analysis is unusually susceptible.

Another problem is that the regional case authors, apparently, were not expected to be either comparative or very theoretical in their approaches. A more explicit and detailed set of common questions might have been helpful in this regard. If you do not start with some theory, it should not be too surprising that you do not end up with much.

Ultimately, however, one must ask whether—or on what basis—it makes sense to compare Southern Africa and the Caribbean Basin as regional systems? Granted, both areas have experienced hostilities that have elicited superpower attention in the 1980s—but is that enough? The editors would add that one region's conflicts seem to have abated (Southern Africa), while the other's (Caribbean Basin) have not and that this differential outcome was a good reason for comparing them.

Yet it is not clear that both areas of focus have much else in common (except perhaps for Cuba). Southern Africa is a genuine regional system characterized by an indigenous regional hegemon (South Africa) that is much more powerful than the other members of the region. The asymmetry of power within the region is not so great, however, that it could not be balanced by external intervention in the form of Soviet and Cuban military assistance.

In contrast, the Caribbean Basin is a dubious candidate for regional system status. While it may have been United States policy to treat the area as a single theater, it is by no means clear that local actors see it that way (a point made by Lloyd Seawar's chapter). However one chooses to draw the regional boundaries, it is clear that the regional hegemon is not Cuba (as the editors curiously suggest) but the United States.

Thus, the power asymmetries are much greater in the Caribbean Basin than in Southern Africa. The regional hegemons are much different. The closest analogue to what the Caribbean and Central America represents to the United States was what Eastern Europe once represented to the former Soviet Union. The nature of external intervention is also much different. It is not a paradox that Cuban military intervention was much greater in Southern Africa than in the Caribbean Basin.

All of these nontrivial differences tend to get in the way of theoretically guided comparison. In the end, we learn something about regional conflicts in Southern Africa and the Caribbean Basin that is both interesting and worthwhile. But we learn much less about how regional systems resolve conflict or manage to survive with or without the intrusions of external powers.

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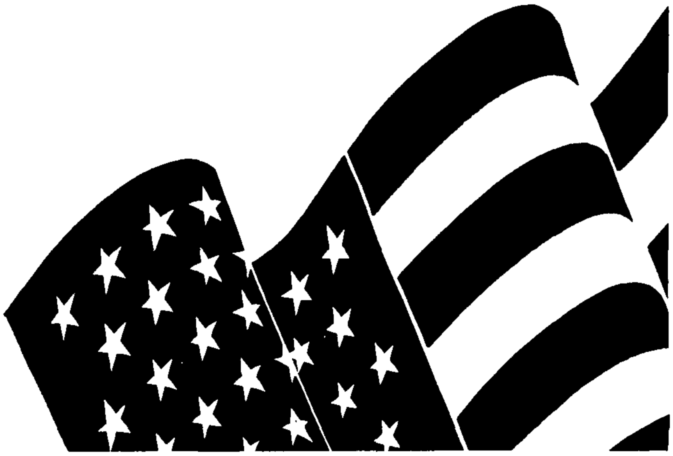

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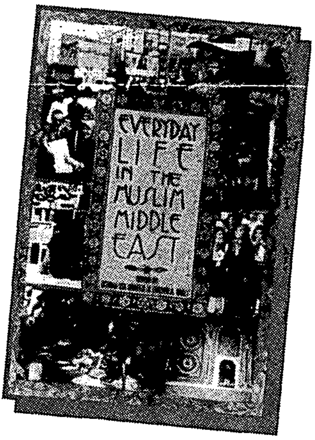

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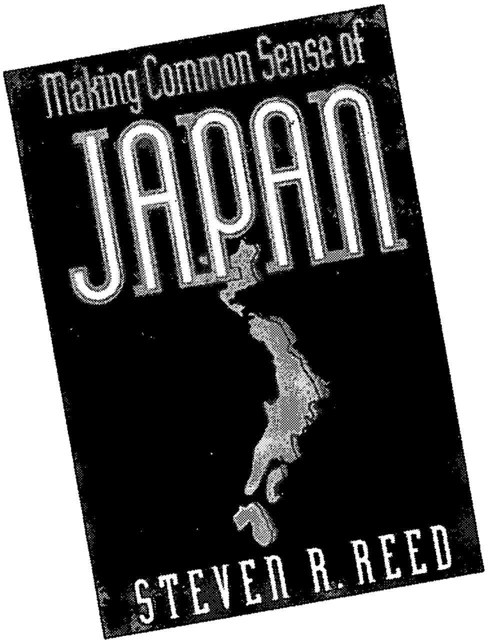

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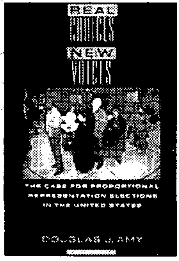
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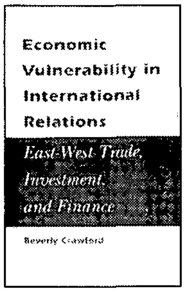

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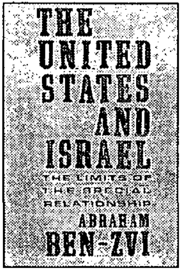
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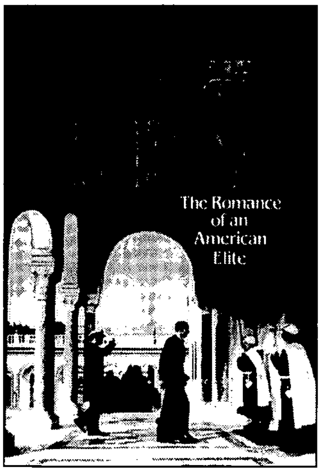

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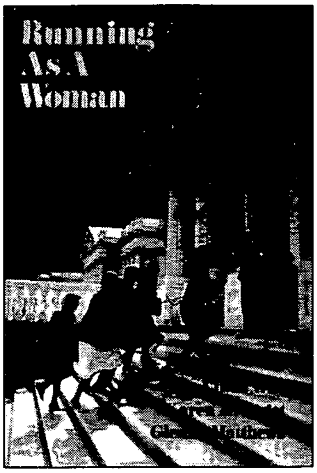

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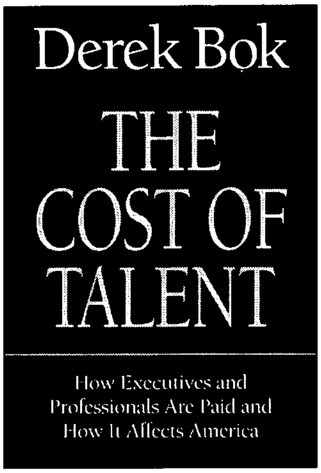
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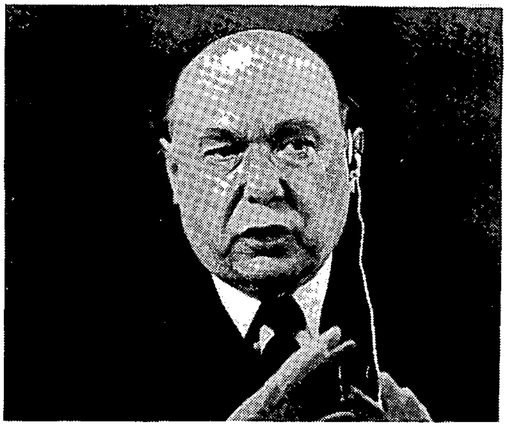

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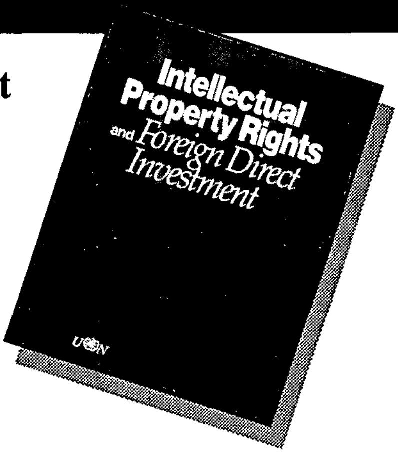

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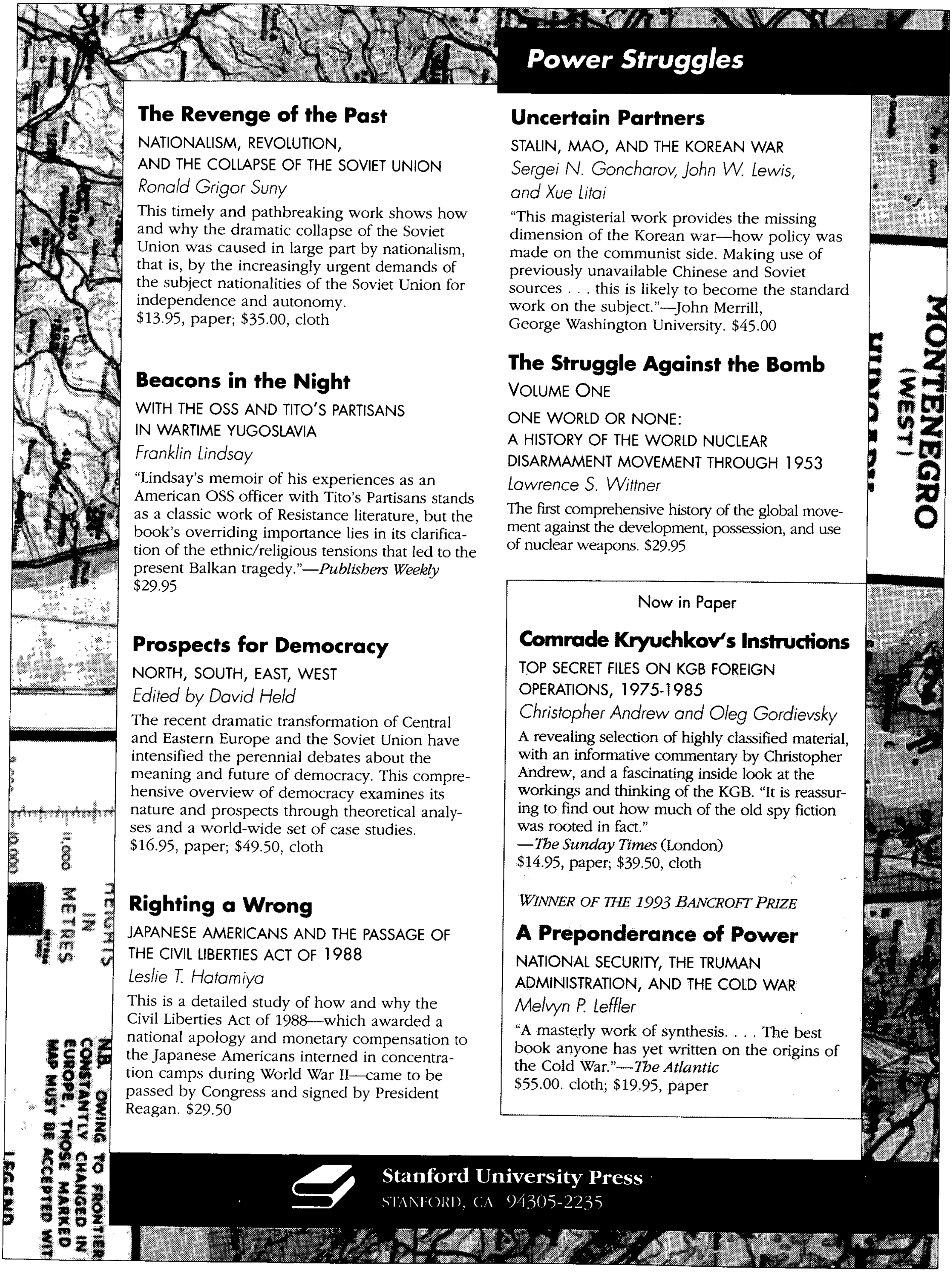

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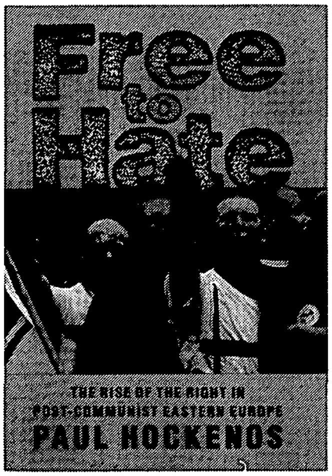

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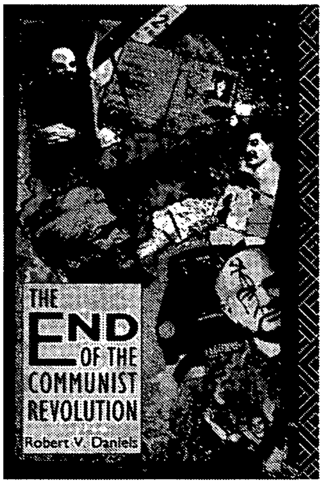

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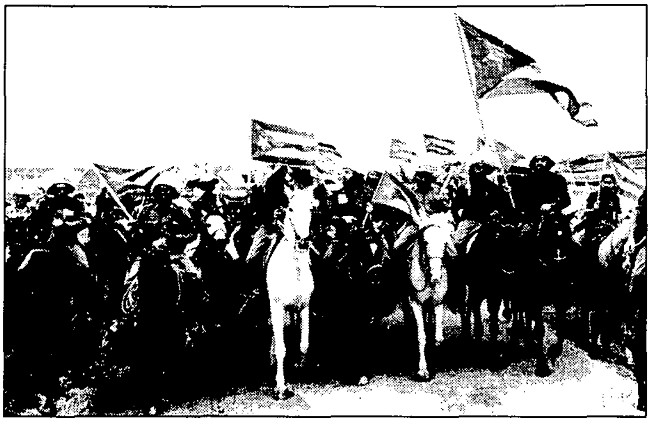

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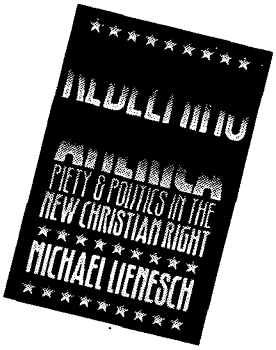

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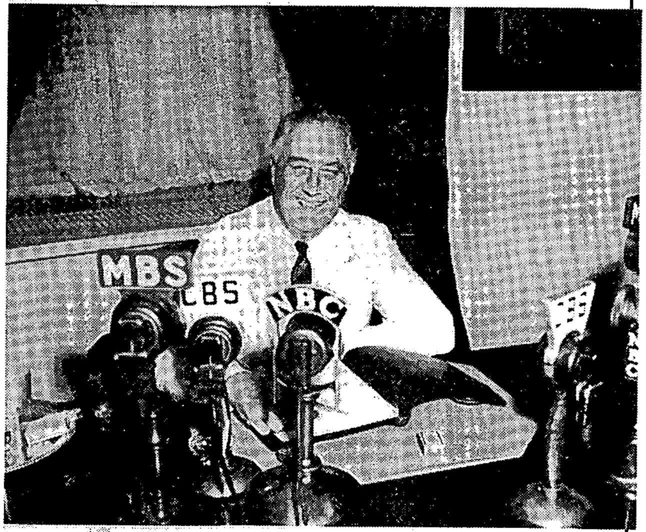

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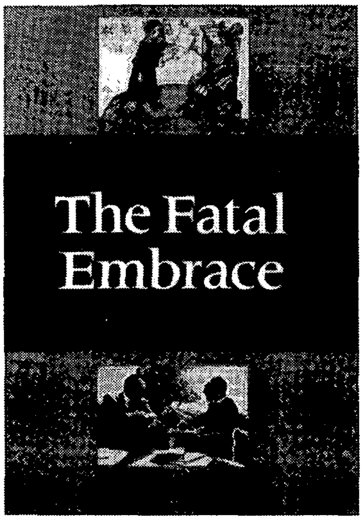

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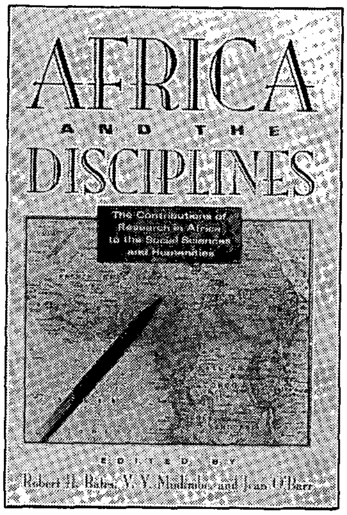
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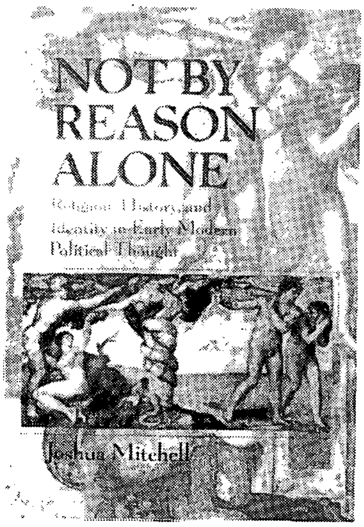
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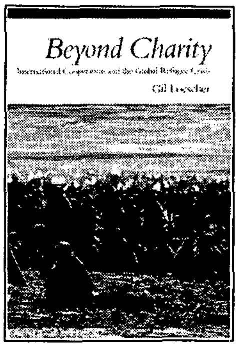

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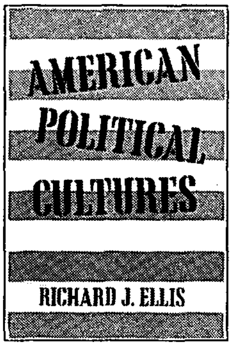

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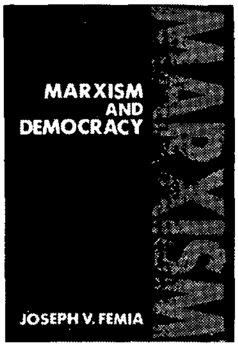

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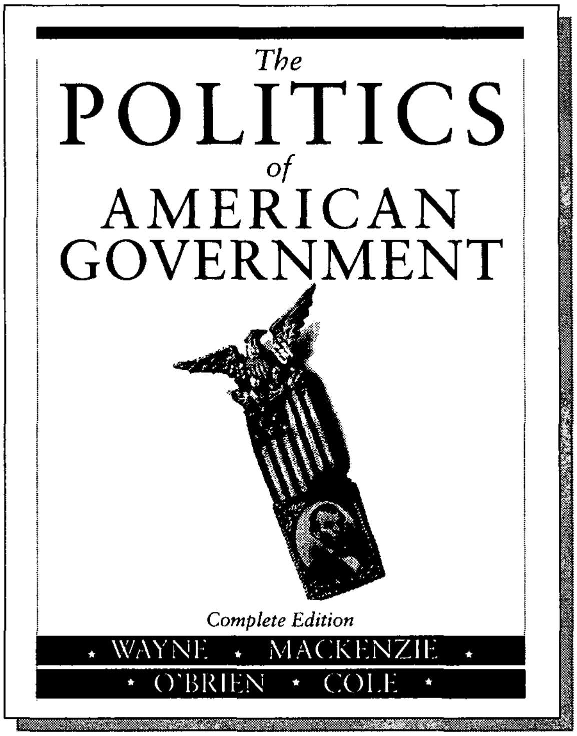

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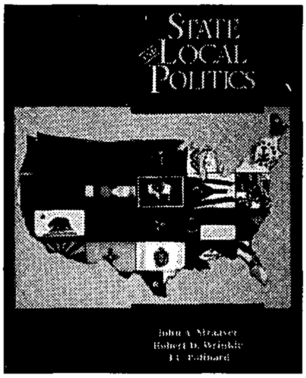
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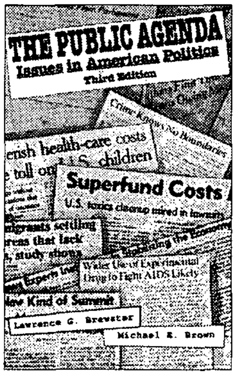
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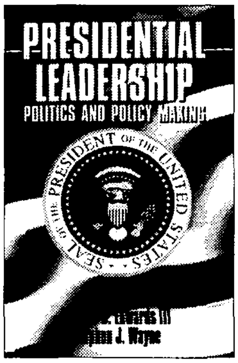
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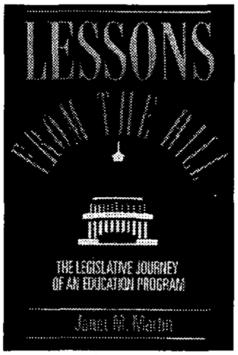
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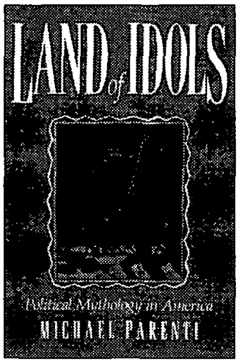
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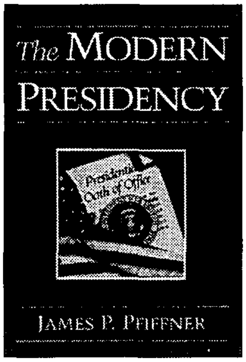
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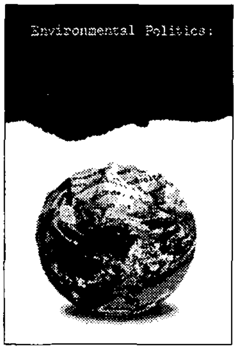
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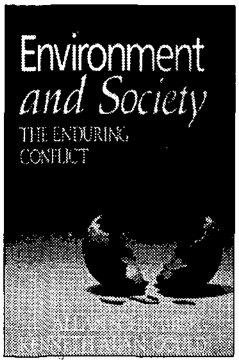
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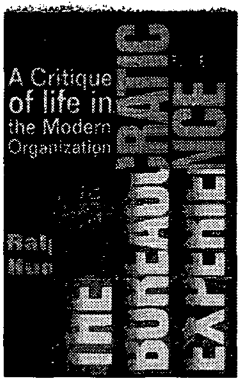
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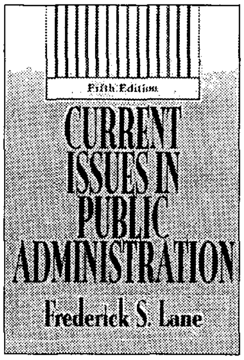
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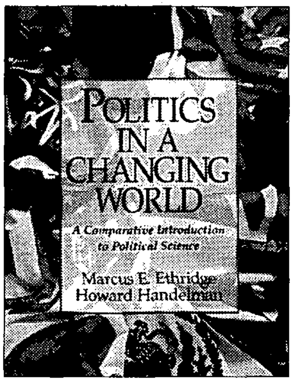
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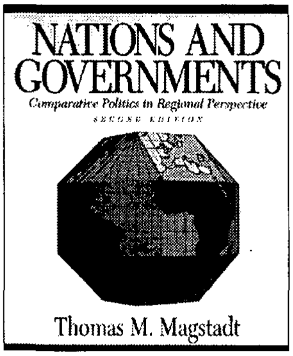
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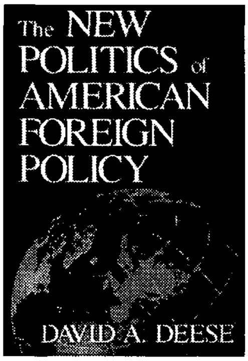
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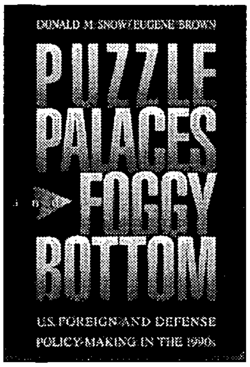
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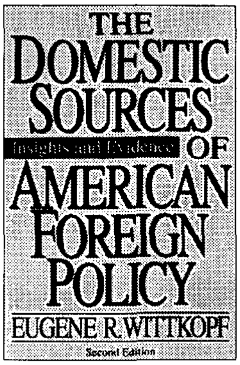
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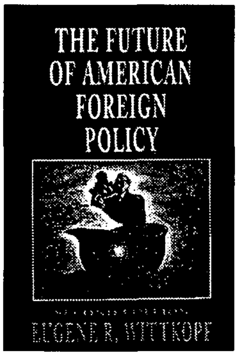
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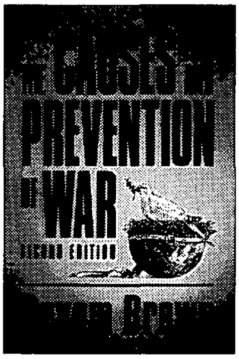
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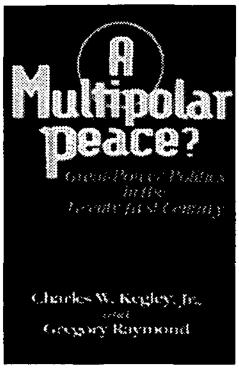
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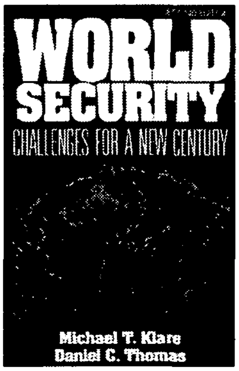
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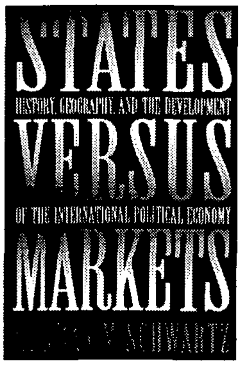
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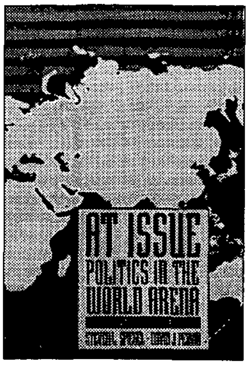
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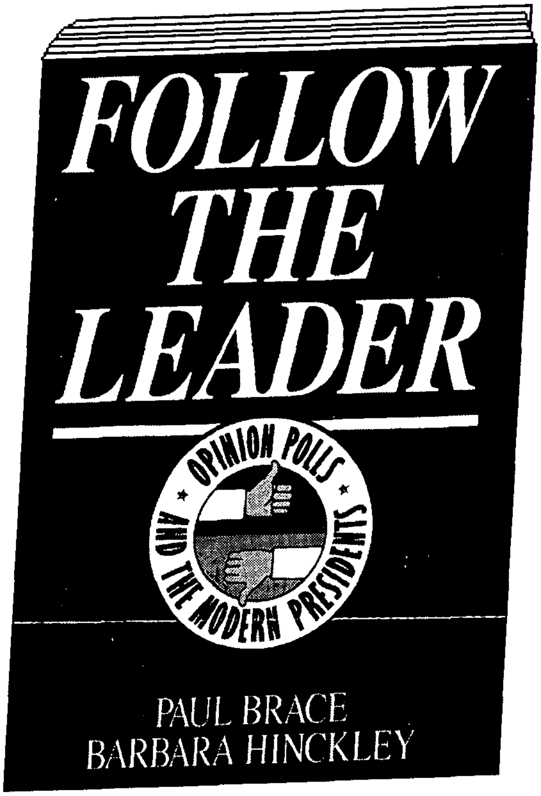

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The Council will elect the next editor at its August 31, 1994 meeting after learning of the Search Committee's deliberations and hearing the recommendation of the President. The new editor will begin mid-summer 1995.

The purpose of this notice is to invite you to submit suggestions for editor to the Search Committee. Please send your nominations to the APSA headquarters by December 15, 1993.

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